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MR. ABERNETHY'S

PHYSIOLOGICAL, PATHOLOGICAL, AND SURGICAL OBSERVATIONS;

DELIVERED IN THE

ANATOMICAL COURSE OF LECTURES,

At St. Bartholomew's Hospital.

Morbid Anatomy.—Now we go to something more to the purpose, and that is the morbid anatomy of the nervous system. Here I first speak of morbid appearances of the dura mater; and yet I say, with regard to morbid anatomy, there 's nothing in it that seems to me to be particularly interesting. The same diseases occur in other parts of the body. The dura mater will be *thickened*; it has an under secreting surface, and the secretions from that surface may be altered in consequence of inflammation. Here you will see actually a *fungous* membrane formed, from a gelatinous substance that has been deposited. It is in some degree organised. It 's what you see in the *larynx*, in the case of *croup*. Well, the dura mater may be thickened, it may adhere to the surface of the brain; and here is a specimen, a very curious specimen it is, in which the dura mater has been separated from the *arachnoid membrane*, to show that it is distinct. I know the history of this case very well. Here are specimens of growths from the under surface of the dura mater. The dura mater may be ossified. And yet, I say, these things are what any body would believe without witnessing the facts; and therefore what appears to me to be of most consequence to us is, that we should consider that the dura mater is the periosteum of the internal table of the skull; and that if there was any thorough disease of the cranium, you might as rationally expect the cranium to adhere to the outer layers, as you might the dura mater to adhere to the under layers, or inner table of the skull. And if the dura mater separates from the under table of the skull, what will happen? There will be some fluid collected between it and the bone; irritation between it and that part of the bone will be kept up; gelatinous matter will be deposited; the deposited matter will become organised; and then, if this irritation be continued, as you know how prone disease is to be propagated along continuing surfaces, the dura mater may become generally affected. I have seen excessive disease of the dura mater thus take place, the origin of it being local, and simply in consequence of its being detached from a diseased part of bone. To show you what I mean, I may tell you what happened to a man in this Hospital: he had a disease in the bones of his head; it was supposed to be venereal; he was salivated; he came here in a very reduced state; ultimately he had symptoms of cerebral irritation, and he died. Now upon taking off the top of the skull it was found that there was a part of the dura mater separated from the bone, and a sort of fungus growing from it. This was from one hemisphere. But upon turning the skull round, and cutting it fully open, we found upon the other hemisphere, under cover of the dura mater, a large quantity of good tenacious pus, which extended every where upon the hemisphere, and adhered to the under layer of the dura mater.

Now it 's of great consequence that you should trepan in these circumstances. Why should you not take away a piece of the bone? It may be dead; the patient cannot feel the operation, and you give a discharge to any pus that may be under the dura mater, thereby exempting it from irritation. But under these circumstances, the dura mater may throw out a fungus; the fungus may come in contact with the under table of the bone, and what then? The bone may be absorbed; the fungus makes its way by progression through the bone, and rises up like a *mushroom* on the man's head, where you may have a violent pulsation. This was called a *cerebral hernia*; and Monsieur Louis, in the 5th volume of the Memoirs of the French Academy of Surgery, has described this case. I now show you a pre-

paration of the case. Mons. Louis, when he described the case, told the proper treatment of it in a very few words; he says, in this case it is your duty to remove the bony circle which surrounds the basis of the fungous growth, and afterwards to destroy the fungus by suitable remedies. But I say to you, if you do remove the circle of bone which surrounds the basis of the fungus, in all probability the fungus will cease to grow; for what occasions fungus to grow, but irritation? Remove the cause of irritation, and the growth of the fungus will cease. There is one famous case of this kind on record, which was attended with complete success, described by Haller; it was the case of a German officer. You know that the dura mater may undergo disease distinct from the brain, or the brain disease distinct from the dura mater.

I now come to the membrane of the brain, and here I may tell you, that you will find the *arachnoid membrane* white and thick; and that you will have water effused, and jelly effused, connecting it with the true *pia mater*. But all those appearances, which were spoken of during the demonstrations and the lectures, cannot well be exhibited in preparations. The morbid appearances fade, or the fluid escapes, or the jelly thickens, so that they are not exhibitable in preparations put up in spirits.

Then I come to the substance of the brain itself; and what do we here meet with? Why we meet with tubercles; not unfrequently tubercles in the cineritious and medullary part of the brain. Of these tubercles Dr. Baillie speaks, and he has said some of them are very small. I show you preparations of strings of them. When cut asunder, they seem to be a little yellow, as if they were about to suppurate. Now I have very frequently found tubercles in people who died of *epilepsy*. But then, would you say that this was the cause of epilepsy? I should say, rather the effect of that cerebral irritation which produced the epileptic fits. Can epilepsy take place without *organic disease* of the brain? that 's the question. The answer is, *unquestionably*. It is answered unquestionably by any man of extensive observation. But few people have that extent of observation, which would qualify them to assert this opinion. I refer you to a case where a man died of epilepsy, which was published, I think, by a Dr. Fraser, and where Sir Astley Cooper was employed to examine the brain. He did examine it with the utmost attention, and he found no organic disease of the brain whatever.

Every variety of nervous affection does occur without any organic disease. But when people have epilepsy without organic disease they generally recover, or may recover. If the irritation which occasioned the epilepsy continues for a length of time, naturally enough, faulty vascular actions will be induced, so that in people who have been long subject to epileptic fits you generally find tubercles. But the subject is a most momentous and important one, and very much deserving of your attention. For my own part, I shall affirm that every kind of nervous malady may occur in the brain, as *functional* disorder, and not as *organic* disease. This is most important, because it leads to the consideration of *madness*; and why should they bleed and purge, and the devil knows what, poor people who are afflicted with madness, as if they have all a disease of the organ, the brain itself? Now here I quote Morgagni; he has got a long list of diseases that he has published where tubercles were found, and among that list are cases of persons who were mad from too hard a brain, from too soft a brain, from a fungus in the brain, from a plexus occurring in the brain, and so on; but here comes the epigram, the concluding sentence, "but all these morbid appearances have I seen without any appearance of tubercles at all, and I have seen tubercles where there have been no such morbid appearances." There 's a body of evidence for you. (Much laughter.) Madness is no cause of these tubercles at all. I say the object we should bind our attention to is, to settle nervous inquietude; to tranquilise the vital actions of the cerebrum; to cut off any cause of irritation affecting it; and I take the putting into order the *digestive organs* to be one grand point towards accomplishing this end. (Laughter.) I tell you honestly what I think is the cause of the complicated maladies of the human race; it is their gormandizing and stuffing, and stimulating those organs to an excess, thereby producing nervous disorder and irritation. The state of their minds is another grand cause: the fidgetting and discontenting yourselves about that which can't be helped; passions of all kinds—malignant passions, and worldly cares, pressing upon the mind, disturb the cerebral action, and do a great deal of harm. There is something of moral treatment in madness: you are to quiet the mind as well as the alimentary organs, and not to keep bleeding and blistering, as if you were to cure all the diseased structure by such measures. Of course, preventing too powerful an impulse of the blood is important, but every thing is to be conducted in moderation.

Now, what do you meet with here? You sometimes find the substance of the cerebrum itself, undergoing a morbid deterioration. I have seen it like thick cream—not at all having its natural smoothness of surface,

when divided. And I remember two apoplectic patients who were examined at this Hospital, about the same time, where the morbid state of the medullary matter of the hemispheres of the brain was found thickened throughout the whole length of it: but I remember too—and that was a thing that particularly fixed itself on my mind, that one of those chaps was the happiest mortal alive; though he had those fits, still he thought he would do very well, and for every body who approached him he had a smile. The other was the most uneasy and desponding dog that could live; though he might be exempt from the fits, yet he had no hope; he felt sure he would die, and nothing was presented to him but what was a sort of misery to his mind. Well, that was a difference of character, I suppose, in the two individuals, but the morbid appearances were the same.

Abscesses.—Then you have abscesses in the brain. And when an abscess is formed in the brain, it is like an abscess in any other part of the body. Here is one, the sides of which are flocculent, and what I may call *villous*. Here is a preparation of one, which, I suppose, held an ounce and a half of matter; this was in the front hemisphere of the brain, and this case is a case I wish to tell you of; for, in considering the practice of surgery, it is a very important thing that you should understand, that there is a considerable *sympathy* between the *contained* and the *containing* parts: this man had his scalp torn off by a cart-wheel which passed against his head; torn off and smashed all to pieces; it could not be replaced. He was not stunned: no other harm was done to him, than that the scalp was torn off. He was brought to this Hospital, and here he lived for a considerable time. The *pericranium* having been stripped off the bone, there was a death of the bone, and the exfoliation of the bone was a very tedious process. There was not much wrong with him; and to tell you the truth, he was not much attended to; he was left by the dresser, and he used to dress the injured part himself. I was going round the ward one morning, and I saw him standing at the end, staring in an extraordinary manner; I went up to him, and said, what's the matter with this man? I felt his pulse; it was very low, and his hands were cold. I got into a sort of rage with the nurse, and desired to know why she had not been more attentive to the man. She said, He did not say he was ill, Sir. Why, he might not have said he was ill; but I am quite sure, I observed, that he could not have eat his food of late. No, he certainly did not eat his food well, but he had not made any complaints. However, he died very shortly afterwards. Now here, from mere irritation on the outer part of the head, an abscess formed in the brain; and when abscesses form there, people go off generally in the way I have told you. They have no warning, they have a failure of the vital powers, and they sink very speedily. O, there are many similar cases on record; but here I call your attention to it, as a case indicating the sympathy that exists between the contents of the cranium and the external parts.

You may have abscess in the medullary matter of the brain. And what does all this show? Does it not show that the organisation of every part of the body is precisely the same, since every part of the body is capable of building up the same sort of disease, of converting the healthy structure into diseased structure? How is it done? Why, by the vessels arranging deposited matter from the secretions of those vessels being wrong, instead of preparing that which they ought to prepare.

Apoplexy.—Well, then, again you have *apoplectic cells*. Here are specimens of plenty of them. Blood-vessels burst upon the brain; the blood gets effused; it clots, and forms cells in the brain, and that is called *sanguineous apoplexy*. Now with regard to sanguineous apoplexy, Dr. Baillie has said, you generally see some alteration in the natural structure of the principal arteries; that they are white, in some degree ossified; and that in this state they are more liable to burst than in the healthy state; that there is a sort of diseased structure in them, which renders them the more liable to burst. Well, now, I say that that's the morbid action of the blood-vessels; that it proceeds from the too forcible action of the blood, and that therefore all this is likely to be observed in the principal arteries, where they go into the brain. Here are specimens of arteries which have penetrated the substance of the brain, ossified. This was the brain of a man who had been long ill, and had had very strong nervous symptoms. A surgeon being employed to examine the brain, he notched his knives, and he had to go on haggling and scratching through the ossified arteries which ran through the substance of the brain. Well, of course the arteries of the brain are liable to the same sort of disease which the other arteries of the body are liable to; and of course diseased arteries are more liable to burst by a forcible action of the blood, than sound arteries. Sanguineous apoplexy also, is more common to the old than to the young, where probably some disease of the arteries has taken place, owing to the too forcible action of the blood-vessels. But the young do likewise have sanguineous apoplexy, in consequence of diseased blood-vessels in the

brain. I have seen it many times, but still it is not common. The young seem to me to be more subject to *nervous* apoplexy, than to *apoplexia sanguinea*.

Now what's to be done in these cases? O, to guard against the forcible action of the heart and arteries; that state of irritation which is the cause of inherent vascular action, must be your object. You say, however, that a vessel gives way, and blood is shed; what does it do? Why it acts as pressure. It generally falls on some part of the body; but a very considerable quantity of blood may be shed, and yet the patient may recover. I may relate the case of a great effusion of blood, where the patient did recover; it was the case of Mr. Baddeley, the player. He fell down in a fit; there was a great effusion of blood, but he recovered, played again on the stage, and then he died in a second fit of apoplexy. I think you may easily know the state of an individual who is liable to sanguineous apoplexy, if you will be observant. To convey to you the state of my mind on this subject, I will tell you the case of an old man with whom I was acquainted, who could get no better *doctor* to attend him than myself; he could not afford to give fees. He was a man of a vigorous constitution; he lived nearly to the age of 70, without having had the slightest illness. He then had occasionally a fit of the gout, and subsequently he had a slight attack of apoplexy. The first fit he had affected his speech, and left him in a state unable to articulate. After some years he had another fit, and this affected one side of his face, but not more. Then again he had a third fit, which left him completely without any motion in one side of the body. It was one of the larger vessels that had burst. He was a man of great vigour, both of body and mind, but when I first knew him, it was just as he was recovering from this third fit. He used sometimes to forget all about every thing that had transpired, and was quite unconscious of all around him. I admonished him about his diet, and about keeping his *bowels regular* (laughter); and I made him an issue in the neck, and his recovery was rapid beyond belief. He soon had all his wits about him; he became able to go forth and walk in his garden, to recount again with accuracy all the adventures and incidents of his past life, and he was very comfortable. As long as I knew him, he had a fit of gout in the winter; and every year, in the summer, he had a violent *throbbing* in the vessels of his head; but he kept his bowels open, and went on tolerably comfortable for five or six years. But I remember one summer time, for he lived in the country, on going into the house, I saw that he looked a little confused; and his wife said to me, I don't know what's the matter with him, I think it may be his stomach. I saw the temporal artery beating violently; I said, bring me a basin, and on puncturing the temporal artery, I got about fourteen ounces of blood from it, and closed the wound. Then I said to him, well Sir, how do you find yourself now? says he, as well as ever I found myself in my life. He got up, threw himself down into his chair with a thump on his bottom; now, said he, before you took away that blood I was obliged to sit down very cautiously, otherwise it would have jarred my head, but now you see what I can do. I say, I think you will have forewarnings enough if you are observant.—*Coughing* hurts the head most excessively where there is a tendency to inflammatory action, and the throbbing of the vessels seems to cause them all to go into a sort of sanguineous apoplexy.

As to the nervous apoplexy, you know that is very curious; but you have cases of nervous apoplexy. It cannot be said that that is occasioned so much from pressure, for no pressure seems to continue, and yet the effects go off. However, this is a subject of discussion which is foreign to our present purpose. I'll tell you, that a little pressure will produce apoplectic symptoms in one person, and will not produce them in another person; and that sometimes a person from pressure will be liable to torpor in the brain—that torpor will go off, the patient recover, and again have the same symptoms from the same degree of pressure.

Well, then, what more have I to say? O there's *hydrocephalus*—effusion of fluid into the ventricles of the brain. But I never thought it worth while to put up a preparation of this kind, for really there is nothing to be seen, but a great quantity of water in the ventricles. You have also disease of the *plexus choroides*; and it's curious, that if you have a disease in the plexus choroides on one side, you generally have the disease on the other side. This is a circumstance that Dr. Baillie has remarked, and here is an example of it.

Now I do not know that I have more to say relative to the facts which we observe in dissecting the brain, when it undergoes organic disease.

LECTURES

ON THE

Diseases of the Nervous System,

BY

DR. CLUTTERBUCK.

Lecture XIV.

On the Prevention and Treatment of Apoplexy.

The *prevention* of apoplexy altogether, as well as of the recurrence of the disease where it has once taken place, depends upon attention to the known causes of the disease, and, as far as possible, the avoiding of them. For this purpose you must revert to a former lecture, where the *occasional* and *predisposing* causes of apoplexy were pointed out, and their mode of operation explained. The object to be aimed at is to avoid all causes that either tend to excite the arterial action of the brain, or which impede the return of blood from it. To the former class of causes belong too intense application of mind, mental emotions, great bodily exertion, external heat, whether applied to the surface of the body generally, or to the head more particularly, (*coup-de-soleil,*) the intemperate use of *strong drinks*, and *opium*—to the latter, a dependent posture of the head, and pressure upon the veins, or any other cause impeding the return of blood from the head. Besides the avoidance of these different causes, recourse may be had to *counter irritation* of various kinds, as by issues or setons, frequent purging may also be enumerated among the means of prevention. Occasional *blood-letting* may be also proper, but only when signs of increased arterial action, in and about the brain, present themselves. Loss of blood is by no means proper for the mere purpose of reducing corpulency, for it tends rather, when moderately employed, to increase this. Restriction in diet, much and laborious exercise, with the avoidance of much sleep, are the only rational means of reducing excessive corpulency.

You may perhaps have remarked, that a common soldier is never overloaded with fat; and yet he feeds well, and is not overworked. This, I think, may be ascribed to the more than ordinary degree of watching he endures.

Of the treatment of apoplexy.—This you will find sufficiently easy, if you bear in mind the general nature of the disease, and the causes inducing it. But first let me caution you against the large and indiscriminate bleeding that is often practised on these occasions. The alarm felt at these times leads to the doing a great deal that is unnecessary, and which is sometimes injurious. Practitioners often bleed their patients without measure, and so profusely as to endanger life itself; but, at all events, with the effect of impeding recovery afterwards. They proceed as if they thought that it was only necessary to carry the evacuation far enough, in order to ensure the accomplishment of their object, forgetting that some of the more immediate causes of the disease, are absolutely incapable of being removed by art. But to understand the proper treatment of *apoplexy*, you must advert to the different causes capable of inducing the disease, as the treatment will vary in each particular case.

1. In the case of *apoplexy* produced by direct pressure on the surface of the brain, as by depressed bone or extravasated blood underneath it, the immediate removal of the cause by surgical means, is the first and principal object; and when this is accomplished, the organ, in ordinary cases at least, immediately resumes its functions; if otherwise, time must be given; for art can do little further towards the cure. The occurrence of inflammation, however, must be guarded against; and for this purpose, *bloodletting* becomes necessary, and, in fact, should hardly ever be omitted. Where the functions of the brain are suspended without any discoverable injury to the bone, or suspicion of extravasation of blood beneath, the symptoms are commonly attributed to *concussion* merely; and here, of late, a different practice has been inculcated, as I before mentioned to you, namely, the use of *stimulants* and *opium*. But such means are doubly objectionable; first, as being altogether incapable of affording immediate relief to the brain, whatever be the degree of injury it may have sustained; and next, as being calculated to induce inflammation, an almost necessary consequence of the mechanical violence sustained. I think, therefore, you will do much better to follow the advice of the older surgeons, Heister, Le Dran, Pott, and others; who endeavoured to protect their patients in such cases against future inflammation, by *bloodletting*; proportioned, of course, to the general strength of the system, and the state of vascular action at the time. If the pulse should be feeble, and the extremities cold, *bloodletting* would not be required: but the return of action in the vascular system should be carefully watched, in order to be checked if necessary.

2. Where blood is extravasated in the centre of the brain, (a circumstance that may be conjectured to have happened in sudden and violent attacks, and where the symptoms have continued unabated for some hours,) it is usual to employ *bloodletting* to a

large extent; repeating the operation again and again, in the expectation that the object will be finally attained. In this way, I have seen from sixty to a hundred ounces taken away, in the space of a few hours. Now there is neither reason nor propriety in this. For you should consider that bleeding, to whatever extent it be carried, can have no immediate effect in removing the blood extravasated in the brain; and, consequently, cannot effectually relieve the present symptoms; while, by weakening the system to too great a degree, it impedes, instead of promoting, the absorption of the effused blood afterwards, the only way in which the object can be effected. What then, you will perhaps ask, is the utility of bleeding at all in such cases? The only useful purpose to be answered by it, is the lessening the arterial action of the brain, upon the temporary excess of which, the rupture of vessels and consequent effusion of blood, primarily depend. By doing this, a further effusion is likely to be prevented. But for this purpose, a moderate abstraction of blood is all that can be necessary, such as will suffice to reduce the action of the heart and general vascular system within moderate limits; when the arteries of the brain will be likely to participate in the effect. If, after having taken away blood to the extent of sixteen or twenty ounces, (or, in strong subjects, thirty,) and having repeated the operation once or twice to the same extent, you find the symptoms continue; and if, at the same time, the pulse has lost considerably in point of fulness and strength; you had better desist from further bleeding, and leave the case to nature. The blood thrown out in the brain will coagulate, and the serum diffuse itself around, so as to lessen the pressure in any one part. The organ will accommodate itself by degrees to the new circumstances in which it is placed, so as to be able, in some degree, to resume its functions.

The blood in these cases, is generally found extravasated in the *corpus striatum*, or in the *thalamus*, or near these; and sometimes from these parts, it bursts into the ventricles. A reason has been given for this being so commonly the seat of the extravasation, which is, the number of arteries that penetrate the substance of these parts, without first subdividing on the *pia mater*, as is the case in other parts. It is very seldom that blood is found extravasated in the substance of the *cerebellum*.

Subsequent improvement in the symptoms can only arise from the gradual absorption of the effused fluids; but this is necessarily a slow process, and probably never perfectly performed. Recoveries, however, do occasionally take place, under apparently these circumstances; but this can only be expected to happen in young subjects. As to other means, they are extremely simple. You should be cautious of giving any thing by the mouth, till you are sure that the patient is capable of swallowing, and even possesses some degree of consciousness; for otherwise there will be danger of suffocating him, by the matters getting into the trachea; or else violent coughing may be excited, which, by interrupting momentarily the return of blood from the brain, may aggravate the mischief. *Stools* may be procured, if necessary, by clysters, till the faculty of swallowing is restored. If the extremites are cold, they should be warmed by artificial heat, or the cutaneous vessels excited by friction. The measures now stated, comprise all that is essential, and you perceive that they are both few and simple. A great deal is done in these cases that had better be omitted, as being not only useless, but injurious. Such are *sinapisms* or the mustard poultice, applied to the feet, without discrimination. These, by the pain and inflammation they excite, soon come to irritate the brain itself by sympathy, so as to increase its vascular action. *Blistering the head*, as is often done, is objectionable on the same ground. The giving *ammonia*, or other active *stimulants*, internally, is likewise improper. Should the patient recover, a recurrence is to be attempted to be prevented, by the means before pointed out.

3. In *apoplexy* following *chronic* disease in the brain, and which is known by long-continued pain in the head, with more or less disturbance of functions, the same general principles of cure apply, though in a modified way. It is still inflammation you are to treat; but of long standing, and attended, probably, with alteration of structure. Consequently, although *antiphlogistic* measures are required, it is to a limited extent only that they are admissible. Small *bleedings*, from time to time, give the best, and, in general, the only chance of relief. Occasional *purgatives*, with a careful avoidance of all causes that tend to excite the brain, will often, by perseverance, accomplish a great deal, in cases of chronic inflammation. But this, of course, will depend upon the actual condition of the part affected; which cannot be satisfactorily ascertained: and therefore our prognosis should be doubtful. A fresh accession of inflammation may occur at any time; (just as in a diseased state of the lungs;) with an aggravation of symptoms that may very probably prove fatal.

4. *Arterial fulness*, the result of increased action in the arteries of the brain, is another cause of suspended functions, or apoplexy. This cause operates by compressing the cerebral veins, and thus impeding the circulation of the organ in the manner al-

ready stated. This variety of apoplexy can no otherwise be distinguished from that produced by extravasation of blood in the brain, than by the temporary nature of the symptoms. If the functions are restored quickly and perfectly, as is sometimes the case, the suspension could not have been owing to extravasation of blood; as such a cause must be permanent. But this cannot be ascertained at first. We ought, therefore, in all cases, to *presume* that the case may be of this description, and should act accordingly; for should we fail to restore the patient, the failure proceeds from causes that we cannot control: the means are still the best that the case admits of. These cases of *apoplexy* arising from arterial excitement and distention merely, without effusion, hold out a fair prospect of being cured by simple *antiphlogistic* remedies; especially *bloodletting*, which ought to be freely used, for the purpose of diminishing the arterial action of the brain; for this will be followed by a diminution of their diameters, and by which the veins will again have room to expand in, so as to receive the arterial blood. Other *antiphlogistic* means should be used in combination with *bloodletting*; and especially the steady application of *cold* to the head; with the view of disposing the distended arteries to contract the more readily. The disposition to this kind of *apoplexy* in certain individuals is sometimes very strong; so that trivial causes, such as close attention of mind, or any mental agitation, is sufficient to confuse and stupefy the patient for a time.

5. That *apoplectic* state which is called *intoxication*, and which is induced by *strong drinks*; and the somewhat analogous state induced by *opium*; may be explained, I think, upon the principle of arterial distention, just laid down. *Alcohol* or *spirit* in every form, exerts a specific influence on the arteries of the brain, exciting them to increase of action, followed by distention, and consequent pressure on veins; thereby, as in other cases, impeding the circulation of blood through the organ. So that the restoration of the *sensorial functions* depends upon the circulation of the brain being restored. Now the first step towards this, is the diminution of the arterial action of the brain, which, in fact, soon subsides of itself to the natural standard, and probably below this, if we may judge from the languor and depression felt afterwards. The change in this case is gradual, requiring usually a few hours for its completion; and in this way the symptoms of *intoxication* are by degrees removed. In ordinary cases, the patient may be left to himself; but where the quantity of *alcohol* swallowed is dangerously great, more active measures are required, which, however, are very simple. The object is, to diminish the action of the cerebral arteries; which may be done by lessening that of the whole vascular system, by a moderate abstraction of blood from the arm; while *cold* should be applied to the head itself. There is probably, no utility in administering drugs internally. Strong infusions of *tea*, and perhaps *coffee*, seem to lessen the effects of *strong drinks* on the brain, and so far may act as remedies. The nausea and headach that are often complained of on the following day, are in general best relieved by a moderate quantity of some *stimulant* applied to the stomach.

The effects of *opium* are so analogous with those of *alcohol*, that the condition of the brain is probably very much the same in both: and if so, a similar mode of treatment would seem to be indicated. In one point, however, a considerable difference has been made. In the case of *opium*, we are directed to tease and worry the patient in every possible way, in order to keep him forcibly awake. But this can only be done, by exciting the brain. If the vascular action of the organ be, as I suppose, already in excess, it is not easy to understand how such treatment can be useful. I am inclined to think, therefore, that it would be best to allow the patient to doze quietly for a few hours, as in cases of ordinary intoxication. As a matter of experience there are no grounds that I am acquainted with, that are sufficient for deciding the question, one way or the other. The rest of the treatment in the two cases, is the same.

Whether the inhalation of *carbonic acid*, or other gases, in respiration, influences the brain by increasing its vascular action, or in some more direct way by destroying the *nervous power*, I shall not pretend to determine. While the action of the heart continues to be performed with much strength, there would seem to be a reason for bleeding to a moderate extent. But, in fact, there is little known respecting the mode of acting of these peculiar poisons. It is doubtless of the first importance to withdraw the patient as quickly as may be from such an atmosphere, and to make him inspire as pure an air as possible.

In *apoplexy* occasioned by an impeded return of blood from the brain, the cause of the impediment must be looked to, and, as far as possible, averted. In the case of disease of any kind in the chest acting in this way, the chance of doing good depends, of course, upon the nature and degree of the disease present. If it be inflammation, either *acute* or *chronic*, or the consequence of these, as collection of serous

fluid, the mode of treatment for the removal of the cause, will be easily understood from what was formerly observed on those subjects. As a means of directly relieving the brain itself, opening the jugular vein seems to offer the greatest prospect of advantage.

Arteriotomy has nothing to recommend it, in preference to *venesection*, but rather the contrary. Morgagni advises opening the occipital veins, as there is a communication of these with the interior of the skull, by means of the foramina at this part.

A *fit of apoplexy* has often been ascribed to repletion of the stomach, either by *food* or *flatus*; these being supposed to operate mechanically, by making pressure upon the descending *aorta*. More, I think, is to be attributed in these cases to the *stimulant* and *narcotic* quality of the *wine* or other *liquors*, usually taken at the same time. A mistake here may prove injurious, by leading to the use of *emetics*, which, it is easy to see, are very capable of doing mischief, by impeding the return of blood from the head; which *straining to vomit* does in a remarkable degree.

The *apoplectic state* induced by recent and acute inflammation in the brain, whether occurring in the form of *phrenitis*, *hydrocephalus*, or *idiopathic fever*, is generally confined to the latter stages of those diseases, and under circumstances very unfavourable to the employment of active measures of any kind. The brain, in these cases, is not only suffering from arterial pressure, but from active inflammation, and that in so advanced a stage, and with such injury to the texture of the organ, and such a destruction of the general powers of the system, as to make it very problematical whether *bloodletting*, or indeed evacuations of any kind, can be sustained. The employment of active *stimulants*, so usual on those occasions, does not seem reconcileable with the nature of the case; nor, as far as my observation goes, is the practice proved to be useful by experience. When any degree of *apoplectic stupor* takes place early, in either of the affections mentioned, immediate recourse should be had to *bloodletting*, and that largely and repeatedly.

The affections to which the terms *lethargy*, *coma*, *carus*, *cataphora*, *catalepsy*, *ecstasis*, and others, have been applied, are all but modifications of the *apoplectic* state, and require to be treated upon the same general principles; which it is unnecessary to repeat. It has been proposed to exhibit *antimony* in some form for a length of time, as an alleged means of overcoming the *apoplectic* tendency; but, after trial, I have seen no reason to believe in the efficacy of such a remedy. We shall next have to treat of *palsy*, and its varieties.

FOREIGN DEPARTMENT.

MORBID ANATOMY.

Acephalocysts.

M. Seville presented to the Royal Academy of Medicine at Paris, some acephalocysts which had been found beneath the arachnoid, in the substance of the encephalon. The patient, æt. 27, laboured constantly under an affection of the head; one day the pains increased, and she suddenly died. There were found under the dura mater several ovoid hydatids, some adhering to the arachnoid, others being only contiguous to it, and sunk in a depression in the brain. There were also several in the cerebral veins, in the corpus callosum the left middle lobe, and the right optic thalamus.

Our readers are perhaps aware, that the acephalocyst consists of a round or ovoid bladder deprived of head and body, varying in size from that of a pea to a walnut. The parietes of the bladder are thin and soft, transparent, whitish, filled with a clear aqueous or albuminous liquid; so that the whole mass very much resembles in appearance the white of an egg. It sometimes happens, that several smaller cysts are seen attached to the external and internal surfaces of a larger. It is still a matter of doubt whether they effect any spontaneous movements. These beings are supposed to reproduce themselves by internal buds or sprouts, and are met with in nearly every organ of the body. Seven or eight different species have been met with. The acephalocysts were first described by M. Laennec, in a paper which forms part of those of the Faculty of Medicine, printed in 1806; and M. Laennec was the first to establish them as a genus. They have not, however, been recognised as a genus, either by Rudolphi or Cuvier. For further particulars, vide Lænnec Traite de l'Auscultation, 2d Ed, Vol. II. p. 4. Rudolphi's Entozoorum Historia, 2d Vol. p. 367, or his Entozoorum Synopsis.

PHYSIOLOGY.

On the Reproduction of the Nerves. By M. Prevost.*

There are, in physiology, few questions of a more general interest, than those of the reproduction of the tissues; unfortunately, in vertebrated animals, the phenomenon which now occupies our attention, is only presented in a certain degree of perfection in the cold blooded animals; in warm blooded vertebrated animals the reproduction of tissues is so very imperfect, that one of the greatest anatomists of this century has denied its existence, with the exception of the cellular tissue. Analogy is evidently opposed to such a conclusion; but the objects which Bichat had constantly under his eyes induced him to doubt, and his imagination yielded to the pleasure of supposing one of the greatest generalities which have any attraction for the human mind. Many distinguished anatomists have made some excellent experiments on unguiculated animals; but their labours, undertaken in a practical point, furnished us with very little light on the organisation of regenerated tissues. Some new facts, examined in a point of view purely scientific, offer, then, some interest, even when isolated, and it is this which induces me to publish the following observation, which relates to the nervous tissue.

Last summer I divided, in five young cats, the left pneumo-gastric nerve; I took off a portion, about six millimeters in length, from this nerve, in order that the superior and inferior ends might be separated by a marked interval. Neither of the cats appeared affected by this operation: the cicatrisation of the wound was very rapid; they continued to feed as if nothing had happened. A month after, I cut, in one of them, the same nerve on the opposite side; the animal appeared to suffer a great deal; it frequently cried out; its respiration became more and more painful, then very slow, and at the end of fifteen hours it died. I examined with care the pneumo-gastric nerve, which had been divided a month; the two ends were swollen; they were directed one against the other; a whitish tissue, very much like the thickened and disfigured neurilema, united them. A person might judge from this circumstance, that this substance did not propagate the nervous action. I then waited a month, and repeated on the second of my cats the same operation; it was stronger, and died just as the former, but only after thirty-six hours. The examination after death presented on the left pneumo-gastric nerve the same substance which we have before spoken of, and which seemed to cause a solution of continuity between the filaments of the superior and inferior portion of the nerve.

There still remained two cats, the fifth having perished by an accident. This time I waited two months, and I dissected the third when it was four months old; it appeared to suffer less, as three days after it was full of life; I decided to divide likewise the right pneumo-gastric on its companion: this did not suffer more than the former. In fifteen days they ran about, and the two cats recovered remarkably well. The question was, if the preservation of their life did not depend on some anastomosis, which had re-established the nervous communication; I therefore re-divided on the first of these the right pneumo-gastric nerve, immediately above the place where it was divided the first time: the animal supported this operation very well; its respiration did not prove painful to it. Thirty-six hours after, I divided on the same subject the left pneumo-gastric, above the former place; at the end of thirty hours the cat died, as if the two nerves of the eighth pair had been divided at the same time.

I dissected with care the nervous trunks that had been divided; the two portions of the left pneumo-gastric were united by a firm swelling, of a whitish appearance, on which the neurilema appeared much thicker than any where else. I broke this swelling, and removed with caution all the thick neurilema which had formed the most external layer; I pressed the remainder between two pieces of glass, and placing it under the microscope, distinctly saw the filaments of the superior nervous trunk extend into the inferior trunk across the interposed substance, thus indicating the restoration of the tissue in its integrity. I examined in the same manner the cicatrix which was established in the preceding division: no filaments could be followed. I repeated in like manner the same experiment on the last of the cats, with a result exactly analagous, and I deduced the following conclusions:—

1st. When a nerve has been divided, it is not sufficient, in order that the action may be re-established, that the two divided portions, as sometimes happens, be united by this white cellular tissue, which is interposed between them, and which adheres to both.

2d. It is necessary, in this interposed substance, that the nervous filaments should extend from the superior to the inferior parts.

* Annales des Sciences Naturelles, Fevrier, 1827.

3d. This prolongation appeared to take place after a very long time. I should add, that the extended filaments were not in that regular juxtaposition which is remarked in the nervous chords; they were, on the contrary, separated, as if they were with difficulty traversing the interposed substance.

For an account of what has been done prior to these researches of M. Prevost, we refer our readers to the foreign articles in the 185th Number, Vol. XI. of THE LANCET.

Modus operandi of Belladona on the Eye.

M. Segalas read a paper before the Royal Academy of Medicine in Paris, on the manner that belladona acts when applied to the eye. Having placed on the eye of a young cat a grain of the extract of belladonna, he saw the dilatation of the pupil begin fourteen minutes afterward. Having, in a second experiment, placed a grain on the pleura of a cat of the same age, and the same height, he saw the dilatation return eight minutes after. Lastly, in a third experiment, having injected a grain into the bronchial vessels, the dilatation appeared two minutes after, that is to say, seven times quicker than after the direct application of the substance on the eye. It would seem, then, says M. Segalas, that the dilatation of the pupil is quick in proportion as the belladonna is applied to a surface possessing a greater or less degree of absorbing power, from which it would result, that the belladonna only acts on the eye after being absorbed and carried by the blood to this organ. He adds, as another proof, that the belladonna, which is only applied to one of the eyes, manifests, nevertheless, its action on both at the same time.

MM. Renauldin and Orfila made a few observations on this subject. The former wished to know, how long the dilatation of the pupil lasted; he saw in one case, where the belladonna had been applied, the power of this substance continue thirty-six hours. The dilatation continued just the same time in M. Segalas's experiment. The latter (M. Orfila) stated, that hydrocyanic acid applied on the eye kills suddenly, and much quicker than when applied on any other part; moreover, death follows so rapidly, that it is difficult to believe that the poison has been absorbed. In the numerous toxicological experiments which Mr. Orfila tried, belladonna was one of the substances, it was applied to the skin, the cellular tissue and the various organs. The pupil was sometimes dilated, at others contracted, and in certain cases it remained insensible; but it constantly became dilated when the belladonna was directly applied to the eye.

ZOOLOGY.

*On the Habits of the Ornithorynchus.**

In a late number of the Anthologia of Florence (Dec. 1826), M. Gregori has published the following observations on the habits of the ornithorynchus. "The ornithorynchus inhabits the marshes of New Holland; it forms among the tuft of reed on the borders of the lakes, a nest composed of brushwood and different roots, interlaced among each other; in this it lays two white eggs, smaller than those of ordinary hens' eggs; it sits on them a long time, and in the same way as birds do theirs, and never leaves them unless menaced by some formidable foe. It appears, that during this time, it eats neither seed nor herbs, but contents itself with the mud or mire close to its nest, and which suffices to nourish it; at least, this is the only substance found in its stomach. When the ornithorynchus plunges under water, it rests there but a short time, and comes to the surface, moving the head in the same way as ducks do. It traverses the sides of the marshes with great rapidity; it is very alert, and is very difficult to take, on account of the excellence

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* Ornithorynchus Mandibulæ rostratæ (anatinæ) Dentes nulli †.

O. Paradoxus. The duck-bill. Ger. das Schnabelthier. (Abbild. Nat. Hist. Gegenst. Tab 41.)

This extraordinary creature is distinguished from all mammiferous animals yet known, by the unprecedented formation of its jaws, which resemble in the closest possible manner, the broad flat bill of a duck, are covered in the same way with a soft membrane, plentifully supplied with nerves for the purpose of tasting, and also, like it, serrated at the lateral edges. The feet are provided with webs, which, in the front ones project beyond the claws, and by that means can be folded up or expanded like a fan. No appearances of teats have been hitherto detected in either sex. This wonderful animal lives near the lakes about Botany Bay, in the fifth part of the world, so rich in creatures of remarkable formation. Vide Blumenbach's Natural History by Gore, a book which we strongly recommend to the younger part of the profession.—

† For the organs which Sir E. Home has described as the molar teeth, and which neither possess enamel nor bony substance, fangs nor alveoli; the structure of which too, he has compared with that of the inner membrane of the gizzard in fowls, cannot, according to the scientific terminology of anatomy and natural history, be considered as actual teeth of a warm blooded quadruped.

of its vision. It only employs one nostril to breath through in the air, which might lead one to think that the other is only used when the animal is under water. It scratches the head and neck with one of its hind feet, in the same way as dogs; it endeavours to bite when taken, but its beak being very flexible and weak can do no mischief. The male alone is supplied with a spur on the hind leg, and this it employs against agressors. The wound which it inflicts produces inflammation and a very severe pain, but there is no instance of death being produced by it."—*Antologia di firenze*, tom 24, p. 305.

We regret that this work, say the editors of the Annales, usually omits to state from what source it derives the curious facts which it communicates to its readers. These which have been just mentioned bear a resemblance to an account of Sir Patrick Hill, printed in the 13th volume of the Transactions of the Linnæan Society of London, p. 622; but they are rather more precise, which leads us to suppose, that they are derived from another source.

We will just quote an extract from the answer of Dr. Southwood Smith, to M. Edwards, in reference to some points which M.G.St. Hilaire wished to ascertain respecting the ornithorynchus. "There is neither at the Linnæan Society, nor any other institution in London the eggs of an ornithorynchus. One preparation has been seen and declared to contain an egg; Sir Everard Home examined it, and declared that it was a mistake. Sir E. H. states, that no one has taken more pains to remove any doubts on the ornithorynchi; he hopes ultimately to succeed, the secretary of the Colonial department having taken some interest in the subject."

The question whether the ornithorynchus be viviparous or oviparous, has been strongly discussed. Geoffroy St. Hilaire and Cuvier contended for the latter point; Blainville and Home for the former. This matter, however, has been satisfactorily settled by the dissection of the animal recently made by Meckel. He has demonstrated the existence of *mammary glands*, and what has an *oviparous* animal to do with such structures? it no more lays eggs than a cow. The story of M. Gregori is a fable.

PHYSIOLOGICAL OBSERVATIONS.

By James Carson, M.D. Liverpool.

(*Continued from page* 142.)

Dr. Bostock in the first volume of his Physiology, published more than two years ago, admits that I had established the doctrine that the blood was returned to the heart in the veins by atmospherical pressure, and not as was previously supposed by the projectile force of the heart and arteries. The Doctor, however, in allusion to the causes by which atmospherical pressure is made to influence the motion of the blood in the veins, says that some of the causes adduced by Dr. Carson are doubtful, and others decidedly erroneous: among the former he ranks the automatic expansion of the heart, in consequence simply of the relaxation of its fibres; and among the latter, those decidedly erroneous, the elasticity of the lungs, but reserves his reasons for these opinions, prudently perhaps, to a subsequent part of his work. I should however have deemed it a fairer method of proceeding if the Doctor had withheld the statement of his objections until he found it convenient to support them by argument, rather than suspend over my head in a shapeless and intangible form for so long a period his threatened refutation, not without biassing, for that time, the public opinion by the weight of his authority. But my long suspense has been abundantly atoned for, by the actual though protracted birth of his conceptions. The Doctor, in a note, for my name is not once mentioned in the body of the second volume, observes, in reference to the resilience of the lungs, as follows: "I conceive that the reasoning of Dr. Carson on this subject is not well founded. His experiments made with a view to ascertain the elasticity of the lungs, he candidly confesses to be imperfect in execution; nor do I think that the mode he employed will be found competent to the end in view. *Phi. Trans. for* 1820. *p.* 20, *et seq.* When the lungs collapse they discharge a portion of air; this will pass into the globe which he employed, and expel a part of the water into the connected tube. But before we can obtain a measure in this way of the elastic force of the lungs, we must ascertain the proportion that their bulk bears to that of the globe, and this, to the quantity of air expelled from the lungs. Dr. Elliotson has, inadvertently, attributed the collapse when the thorax is opened, to the same cause which produces ordinary respiration." The compliment for candour which the Doctor is pleased to bestow upon me, I, perhaps

ungratefully, but certainly totally, and in the most unlimited sense, disclaim. I do not, nor ever did admit, that my experiments were imperfect in the execution. The apparatus employed was not sufficiently large to ascertain the elastic force of the lungs of an ox, in that full stage of dilatation in which they must exist in the living and sound system; but it was fitted to expand the lungs to such a degree, as that in which their resilient force supported a column of water eighteen inches high. In all the lesser animals, it was calculated to ascertain the elasticity of the lungs at their full dilatation. The deficiency might easily have been remedied, but seemed so unimportant (completely so with respect to my argument) as not to demand another apparatus. For if the lungs possessed such a resilient force when they were not fully dilated, they must have possessed a greater when they were so. The size of the globe, the quantity of air that might be in the lungs, and the proportions between their bulks, had nothing to do in the matter. The simple object was, to bring against the internal surface of the lungs, air under additional pressure, and to increase this pressure till the lungs were distended to the volume which they occupy in the system. It is the degree of pressure, not the quantity of air compressed, that gives efficacy to the experiment. It is upon this very erroneous view of the subject, that Dr. Bostock presumes to accuse Dr. Elliotson of having inadvertently attributed the collapse of the lungs to the same causes which produce ordinary respiration. I can safely leave Dr. Elliotson to defend himself. He is not an author whom I would suspect of being guilty of publishing errors from inadverteney. It is with pleasure, however, that I embrace the opportunity which the mention of his name gives, of expressing my acknowledgments to Dr. Elliotson. It has been the fate of my doctrines, a circumstance perhaps to be expected from their novelty, and the obscurity of the source from which they proceeded, to be carelessly examined, frequently cruelly misrepresented and ignorantly condemned. But soon after the publication of my inquiry into the motion of the blood, I was indebted to Dr. Elliotson, for a candid, faithful, and luminous exposition of the doctrines it contained; and the favorable sentiments then expressed, have been repeated by that gentleman in subsequent works.

In the first volume of his Physiology, Dr. Bostock ranks among the doubtful parts of my doctrine, the expansion of the chambers of the heart from simply the relaxation of its muscular fibres; and advanced as an original conjecture of his own, that the expansion, so far as it may proceed from its own mechanism, was produced by an elastic substance which he conceived to be an ingredient in the structure of the heart. This opinion is not repeated in the second volume. No one who takes the trouble to compare what Dr. Bostock has said on this subject, with what is contained in the 64th page of my inquiry, will be disposed to allow the Doctor much praise for originality in this instance.

Dr. Barry, an English physician resident in Paris, published, in the year 1825, a memoir, in which he asserts that he had discovered and established by positive proof, that the blood in the veins is returned to the heart by atmospherical pressure. At the commencement of the English copy of the same work, he evidently seems at a loss how to dispose of me, as I could not but appear to be in the way of his pretensions, for the same doctrine had been published, and, as I supposed, established by sufficient proof ten years before Dr. Barry's work appeared. To pass me over in silence, might seem to argue a want of candour or of information. He thinks it more prudent to set me aside gently; which he does in the following brief notice. "The doctrine of active resiliency of the lungs, tending constantly to leave a vacuum between their surfaces and the parietes of the abdomen, and thereby assisting to bring uninterrupted atmospherical pressure upon the blood in the veins, was, I believe, first broached by Dr. Carson of Liverpool, in 1825. Being, however, purely theoretical, and unsupported by direct experiment, it seems, notwithstanding its ingenuity, to have made but little impression; for although published now ten years, I do not find it alluded to in the lectures and writings of the French Physiologists."

On this passage I may be allowed to make a remark or two. My doctrine is purely theoretical, and unsupported by direct experiment? In this case, if true, it can only be received in the way of a fortunate guess. But I maintain, that theory without experiment is at least as sure a way of reaching truth, as experiment without theory, or in other words without reasoning. I do contend, that there is not a more abundant source of that confusion and error by which physiological subjects are loaded, than the multitude of inaccurate conflicting experiments, and the deductions so confidently made from them. These experiments are generally pompously marked, 1st, 2d, 3d, and so on, and then come the oracular inferences, which are numbered in the same way, and the list is so long that before you come to *lastly*, you have often to count as many heads as are usually found in a Cameronian sermon. It is perfect heresy to dispute any of these deductions, however con-

trary they may be to reason or common sense, because forsooth they are founded on experiment. But I appeal to the unprejudiced and competent judge, if my reasoning on the motion of the blood does not rest upon a sure foundation. The field of observation on which my argument is founded, has been extended indeed more frequently by the experiments of others than my own. I preferred the former when they could be obtained on good authority and applicable to the purpose. But are the experiments which I made to prove the elasticity of the lungs, and which are printed in the Philosophical Transactions for 1820, neither direct or of no account? If these experiments are permitted to be correct, and I challenge the world to prove the contrary, I can have no fear respecting the fate of my theory.

But my opinions have not been noticed in the writings and lectures of the French Physiologists. How can that circumstance, admitting it to be true, affect their value? It would have been well I think if the opinions of some persons had met with the same fate as mine have done in this respect. The flattery of the deputation of the Institute, appears to be an unction not altogether safe in its application to the brain. I have had little opportunity of being acquainted with the recent physiologists of France; a circumstance which I regret. My name however must have been long known to Magendie, if I may judge from the remarkable coincidence between many of his doctrines, and those contained in the inquiry into the causes of the motion of the blood. My experiments respecting the elasticity of the lungs, are admitted to be conclusive by Adelon; and it is the more surprising that such an admission should come from a physiologist who advocates the inapplicability of mechanics in any case to the motions of the living system, and who finds the ready method of solving all difficulties by referring them to the unknown and inexplicable operations of the *organique vitale*.

But my writings, it appears, have "made little impression." From what has it arisen that the causes of the motion of the blood and the mechanical part of respiration, subjects which had for a long time been supposed to have been fully explained, and on which the prevailing doctrine, though it might occasionally raise a doubt in the mind of an inquisitive student, had been generally admitted with implicit belief, have lately, in all the great medical schools of this country, and particularly in the liberal and enlightened University of Glasgow, been agitated with a sceptical intensity and freedom, which a few years ago would have been deemed presumptuous heresy? To what cause has it been owing that the blood is now generally believed to be returned to the heart in the veins, not by a vis a tergo, but by an abstraction of resistence in front, produced in one way or another? To what cause has it been owing that the office of absorption, which since the predominance of the physiology of the Hunterian School, had been believed to be performed solely by a peculiar set of vessels called lymphatics, is now chiefly attributed to the red veins? To what cause has it been owing, that the lymphatics are supposed to enjoy their absorbing power, to their being a ramification of the same pump by which the blood is returned from the extreme vessels to the heart? To what cause has it been owing that the ascent of the diaphragm in expiration, it is believed, may be, and is generally effected, without the clumsy, and in this case mysterious, action of the abdominal muscles? And all this before the name of Barry was heard of! And indeed it may be asked, to what cause has it been owing that the idea of the venous circulation having been influenced by atmospherical pressure ever entered the mind of Dr. Barry?

Dr. Barry having somewhat unceremoniously set me aside, goes on to say, or at least uses words that imply, that nothing had been added to our knowledge of the circulation, since the days of the immortal discoverer, Harvey. The plain meaning of this is that the circulation of the blood, though discovered, had been left in a very imperfect state by Harvey, and that in that state it had remained until the appearance of Dr. Barry, who took up the work where Harvey left off, or failed, and had the glory of putting a finishing hand to it.

Let us now consider the pretensions of Dr. Barry; I am happy to say, that a few words only will be required for this purpose. By means of a well-contrived apparatus, Dr. Barry found that during inspiration, a vacuum, to use his own language, was produced in the chest, or that the atmospherical pressure upon the fluids within the chest, was diminished during this process. I admit this to be true, and that great praise is due to Dr. Barry for the additional proofs he has adduced in support of this important fact. But I cannot admit, that the return of blood to the heart by the veins is occasioned or even aided by the vacuum thus produced. For what are the channels through which the venous blood can be admitted into, or distributed through, the cavity of the chest? These are the venæ cavæ, and the right side of the heart. With respect to the heart, the portals of this organ do not open and shut in correspondence with the movements of respiration. It might, and no doubt often does happen, that the right auricle is con-

tracting at the moment it ought to receive a fresh supply in consequence of inspiration. The passage of the blood through the heart, is neither quickened or retarded by respiration. But it may be said, that the blood is accumulated in the great veins by inspiration, from which veins the heart, as it requires, draws its supplies. Such a reservoir can only be formed by the pectoral cavæ. But these vessels, in consequence of being placed within the chest, and always under the expanding influence of the elasticity of the lungs of its demicartilaginous structure, are always distended to their utmost capacity, and no additional blood can be drawn by inspiration into a vessel already full. That the pectoral cavæ are always in a state of rigid tubular distention while the lungs are uncollapsed, I had a good opportunity lately of ascertaining, in examining the manner used in bleeding the ox by the butcher. The slight movements observable in the large venous trunks, correspond not with the alternations of respiration, but with the movement of the heart. The supply of blood within the chest is nowhere augmented by inspiration. But the very experiments from which Dr. Barry draws his inference, prove to my judgment that the motion of the blood to the heart is not accelerated by inspiration. For, if the whole blood that was required during a complete respiratory process, were brought into the chest during inspiration, along with the air entering by the windpipe, it might be supposed to be sufficient to fill the additional room, made by the extension of the boundaries of the chest by inspiration, and the power of sucking from other sources, as through the instrument inserted into the jugulars of a horse, would be destroyed, particularly as gravity in the position of the animal, and indeed in any position of it, would favour the flow of blood from the system, in preference to the rising of a fluid against gravity, through the spiral tube. The rising of the blue liquid, could only take place in consequence of the impediments already mentioned, to the irregular admission of blood into the chest, which inspiration would otherwise in a dangerous manner occasion.

That, however, I may run less risk of being misunderstood, I shall endeavour to account for the result of Dr. Barry's experiment at greater length.

During inspiration, the boundaries of the chest are enlarged. This enlargement must be accompanied with an increase in the size of the vessels which the chest contains; and the increased size of these vessels cannot take place without the admission of additional matter from without the boundaries of the chest. The only channels through which the admission of additional matter into the vessels of the chest can take place, are the windpipe and the vena cava. It plainly results from the position and mechanism of the cava, that this vessel is at all times fully distended with blood, or that it at all times contains the same quantity of this fluid. It is through the heart alone that the blood is transmitted from the cava to the other vessels of the chest, the pulmonary vessels. But in a given time the heart discharges as much blood out of the boundaries of the chest, as it receives from the cava. In ordinary circumstances, the quantity of blood contained by the vessels within the chest is at all times the same. The substance required to fill the vessels enlarged by inspiration, must be chiefly derived from the air entering the windpipe; but when the bronchia are distended to a considerable degree, the elasticity of the structure opposes a degree of resistance to further distention. In consequence of this, all the contents of the chest are released during full inspiration from a portion of atmospherical pressure. The blood in the pulmonary vessels sustains at that time less than the usual weight. From this circumstance spring some of the most important uses of respiration. Air enters some of these vessels, while an ebullition of matter from the blood takes place in others, which occupies by its change of state a greater space. There is, during inspiration, a demand for matter greater than can be supplied. In this manner this ascent of the blue liquid in Dr. Barry's valuable experiment is fully explained. This subject is discussed at great length in a paper on the Sources of Animal Heat, which I read more than two years ago, in the Literary and Philosophical Society of Liverpool, which has not been printed, but which will soon, in all probability, be laid before the public. The opinions advanced in this paper receive no small support from the ingenious experiments of Dr. Barry. But supposing the blood in the whole venous system to be put in motion by inspiration, and stopped again at the commencement of expiration; what commotion would be observable in the venous system! what immense pulsation, beating time with inspiration! Some people, particularly those who gain a living by diving, can refrain from inspiring, I believe a minute or two. What becomes of the circulation during that period? Besides, the important question, what expands the heart after contraction? is left untouched by this hypothesis, which I think will require a few more reports from the Institute to establish.

Dr. Hastings, and many other physiologists, maintain, that the elasticity of the lungs is not a necessary requisite for circulating the blood, and therefore not likely to be relied upon by Nature, generally econo-

mical in the means she uses, as the blood circulates perfectly in birds, fishes, insects, and in the fœtus in utero, in which there are either no lungs, or lungs without elasticity, or so situated that that elasticity cannot act. To this objection, I have only to reply, that the treatise on the causes of the motion of the blood in the veins, was composed in reference to mammalia after birth alone; that Nature accomplishes the same object by different means, drawn from her inexhaustible store-house, and wisely varied according to circumstances; that fishes and the fœtus in utero being perpetually immersed in a fluid nearly of the same specific gravity with the fluid to be circulated, the same agent would not be fitted to the motion of the blood in animals so situated, that are required for the air-invested mammalia; that in birds which have to perform rapid flights in the higher regions of the atmosphere, the frequent repetition of respiration, as it is performed by the mammalia, would be too laborious, and indeed impracticable, and in a great measure incompatible with the requisite buoyancy; and that in insects the circulating organs are simple in conformity with the inferiority of the functions, and adapted to a life that is to be maintained where air is scarcely to be found, or what we would term impure.

To fail in detailing all the plans which Nature may use for producing the same effect in circumstances so varied, ought not to be produced as an argument against any pretended success in the elucidation of part of those plans. Many animals move, and rapidly too, without legs; yet it does not follow that legs are of no use to the animals who possess them. I further maintain, that my arguments apply only to the mammalia in health; for Nature, in disease, is ingenious in supplying deficiency, and in contriving new expedients.

"Cum enim de morbosis eventibus dicant neque de integri corporis phœnominis, hi quidem viri non possunt contra nos excitari."—*Haller Physiol.* tom. iii. p. 127.

Mr. Spry contends, that some experiments which he lately made, prove that the blood is returned with its usual rapidity in circumstances in which suction could have no influence, and in which the action of the heart and arteries alone could be engaged. The experiment on which he chiefly relies, is to this effect:—The right side of the chest of a dog was opened, and the inferior cava, after it had entered the chest, was bound firmly by a ligature, while the animal was held up by its fore legs perpendicularly to the ground. Blood flowed in a rapid and continued stream from the wound, until the animal died. The vessels of the belly were all found empty, and the liver and abdominal viscera pale. Mr. Spry, in my opinion, is not warranted in concluding from this experiment, that the blood was discharged from the wound in the cava in consequence of the action of the heart and arteries. The refutation of one doctrine does not necessarily establish another. Besides, the very appearances described by Mr. Spry prove, that the blood could not be discharged by the force of the heart and arteries. It must, according to this supposition, be discharged from a hole at a considerable height in opposition to gravity. The veins, at least, below the orifice would all, in that case, be greatly distended with blood. The effect may be explained, on the principles for which I contend. I stated formerly, in a treatise on the circulation of the blood in the head, printed in the Edinburgh Medical and Surgical Journal, that the animal frame may be regarded as a single vessel filled with fluid; that this fluid is not free, but is contained in a net work of tubes that intersect and communicate with each other in an infinite variety of ways; that these tubes proceed from roots, the branches of which continue for some time distinct, but afterwards form communications, not only with the branches of a particular root, but with those of all the other trunks. No one can form an idea of the frequency of these anastomoses, or indeed of the extent of the vascular system, who has not examined the body of an animal which had been killed by the free and permanent admission of the air upon the external surface of the lungs. If we keep before us this view of the vascular system, we shall have little difficulty in explaining the result of Mr. Spry's experiments. It may, in the first place, be remarked, that the destruction or derangement of the circulating powers, made by opening the right side of the chest, and tying the inferior cava, are so great, that inferences drawn from this experiment must be cautiously applied to the healthy circulation. The heart, in this case, was supplied only from the superior cava. That vein was fitted to draw its resources from the whole system, not only from its own more distant ramifications, but also from the trunks of the inferior cava by anastamosis; for if the opening had not been made below the ligature, by which the permeability of the cava was destroyed, life would have continued, at least, for some time. All the blood discharged from the left ventricle, whether it took the course of the ascending or descending aorta, would, in that case, have found its way to the heart through the superior cava. But when the outlet was made in so large a vessel, the direction of the fluid would be naturally altered. Supposing

tracting at the moment it ought to receive a fresh supply in consequence of inspiration. The passage of the blood through the heart, is neither quickened or retarded by respiration. But it may be said, that the blood is accumulated in the great veins by inspiration, from which veins the heart, as it requires, draws its supplies. Such a reservoir can only be formed by the pectoral cavæ. But these vessels, in consequence of being placed within the chest, and always under the expanding influence of the elasticity of the lungs of its demicartilaginous structure, are always distended to their utmost capacity, and no additional blood can be drawn by inspiration into a vessel already full. That the pectoral cavæ are always in a state of rigid tubular distention while the lungs are uncollapsed, I had a good opportunity lately of ascertaining, in examining the manner used in bleeding the ox by the butcher. The slight movements observable in the large venous trunks, correspond not with the alternations of respiration, but with the movement of the heart. The supply of blood within the chest is nowhere augmented by inspiration. But the very experiments from which Dr. Barry draws his inference, prove to my judgment that the motion of the blood to the heart is not accelerated by inspiration. For, if the whole blood that was required during a complete respiratory process, were brought into the chest during inspiration, along with the air entering by the windpipe, it might be supposed to be sufficient to fill the additional room, made by the extension of the boundaries of the chest by inspiration, and the power of sucking from other sources, as through the instrument inserted into the jugulars of a horse, would be destroyed, particularly as gravity in the position of the animal, and indeed in any position of it, would favour the flow of blood from the system, in preference to the rising of a fluid against gravity, through the spiral tube. The rising of the blue liquid, could only take place in consequence of the impediments already mentioned, to the irregular admission of blood into the chest, which inspiration would otherwise in a dangerous manner occasion.

That, however, I may run less risk of being misunderstood, I shall endeavour to account for the result of Dr. Barry's experiment at greater length.

During inspiration, the boundaries of the chest are enlarged. This enlargement must be accompanied with an increase in the size of the vessels which the chest contains; and the increased size of these vessels cannot take place without the admission of additional matter from without the boundaries of the chest. The only channels through which the admission of additional matter into the vessels of the chest can take place, are the windpipe and the vena cava. It plainly results from the position and mechanism of the cava, that this vessel is at all times fully distended with blood, or that it at all times contains the same quantity of this fluid. It is through the heart alone that the blood is transmitted from the cava to the other vessels of the chest, the pulmonary vessels. But in a given time the heart discharges as much blood out of the boundaries of the chest, as it receives from the cava. In ordinary circumstances, the quantity of blood contained by the vessels within the chest is at all times the same. The substance required to fill the vessels enlarged by inspiration, must be chiefly derived from the air entering the windpipe; but when the bronchia are distended to a considerable degree, the elasticity of the structure opposes a degree of resistance to further distention. In consequence of this, all the contents of the chest are released during full inspiration from a portion of atmospherical pressure. The blood in the pulmonary vessels sustains at that time less than the usual weight. From this circumstance spring some of the most important uses of respiration. Air enters some of these vessels, while an ebullition of matter from the blood takes place in others, which occupies by its change of state a greater space. There is, during inspiration, a demand for matter greater than can be supplied. In this manner this ascent of the blue liquid in Dr. Barry's valuable experiment is fully explained. This subject is discussed at great length in a paper on the Sources of Animal Heat, which I read more than two years ago, in the Literary and Philosophical Society of Liverpool, which has not been printed, but which will soon, in all probability, be laid before the public. The opinions advanced in this paper receive no small support from the ingenious experiments of Dr. Barry. But supposing the blood in the whole venous system to be put in motion by inspiration, and stopped again at the commencement of expiration; what commotion would be observable in the venous system! what immense pulsation, beating time with inspiration! Some people, particularly those who gain a living by diving, can refrain from inspiring, I believe a minute or two. What becomes of the circulation during that period? Besides, the important question, what expands the heart after contraction? is left untouched by this hypothesis, which I think will require a few more reports from the Institute to establish.

Dr. Hastings, and many other physiologists, maintain, that the elasticity of the lungs is not a necessary requisite for circulating the blood, and therefore not likely to be relied upon by Nature, generally econo-

mical in the means she uses, as the blood circulates perfectly in birds, fishes, insects, and in the fœtus in utero, in which there are either no lungs, or lungs without elasticity, or so situated that that elasticity cannot act. To this objection, I have only to reply, that the treatise on the causes of the motion of the blood in the veins, was composed in reference to mammalia after birth alone; that Nature accomplishes the same object by different means, drawn from her inexhaustible store-house, and wisely varied according to circumstances; that fishes and the fœtus in utero being perpetually immersed in a fluid nearly of the same specific gravity with the fluid to be circulated, the same agent would not be fitted to the motion of the blood in animals so situated, that are required for the air-invested mammalia; that in birds which have to perform rapid flights in the higher regions of the atmosphere, the frequent repetition of respiration, as it is performed by the mammalia, would be too laborious, and indeed impracticable, and in a great measure incompatible with the requisite buoyancy; and that in insects the circulating organs are simple in conformity with the inferiority of the functions, and adapted to a life that is to be maintained where air is scarcely to be found, or what we would term impure.

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the blood discharged through the ascending and descending aorta to have reached the terminations of the arteries, at which point the projectile power is supposed to have ceased, the blood entering the veins would have its course directed by two powers, which would, in the supposed circumstances, be frequently opposed to each other; these are atmospherical pressure and gravity. Atmospherical pressure would direct the course of the blood towards the superior cava, while gravity would draw it towards the ground. If an opening be made in a vessel, out of which the blood has a ready access, gravity, as it may prevail over the suction of the heart and lungs, in their present circumstances weakened, will cause the blood to flow out of this opening. It is no matter at what point the opening be made, or in what position the body be placed. It is only necessary that the opening afford a free exit to the fluid, and that it be surmounted in height by some part of the body. The heart, however, continues to draw a certain supply, often, indeed, through a circuitous and impeded rout, as long nearly as any fluid is to be found in the system, and raises it from the lowest part to be discharged by gravity out of the orifice in the vein. In this manner will the system be drained of blood, and the vessels of the larger circulation completely emptied, and all the surfaces will be pale. All the appearances noted in Mr. Spry's account of his experiments, will be satisfactorily explained. The influence which atmospherical pressure has in circulating the blood, will not be drawn from the high place which it holds among the powers engaged in that important operation, by these experiments of Mr. Spry. I fear, too, that the hope which Mr. Spry entertained, that for the sake of humanity to animals, his experiments would be the last that would be required, has little chance of being realised.*

Liverpool, April 26, 1827.

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* Dr. Carson has here made a mistake. By referring to No. 179 of THE LANCET, it will be seen that Mr. Spry did not express himself as Dr. Carson has represented. At the conclusion of the debate, *Dr. Clutterbuck* said, "that he considered the matter so far set at rest, that any further experiments, attended, as they necessarily were, with considerable suffering to animals, were unnecessary, and therefore unjustifiable."—ED. L.

LONDON UNIVERSITY.

To the Editor of THE LANCET.

SIR,—As I perceive there is to be a university established in London, and that the conductors of it are men of known liberal principles, allow me, through the medium of your publication, to suggest the propriety of connecting with the London University some great and comprehensive preparatory school of medicine, where young men intended for the practice of medicine in all its branches might receive an appropriate, a classical, and at the same time a cheap education, and thereby avoid the dreadful and really unnecessary expenses which an education at our universities entails on the students. The cheapest kind of classical education at Cambridge costs nearly 300*l.* a year, if a young man desire to live like what is called a gentleman; whereas at the great catholic schools 100*l.* a year covers every expense, and a more liberal and scientific education is there to be obtained. Besides which, all the disorder of excessive drinking and idle waste of time are carefully guarded against. I do not know whether the circumstance may not, in some measure, account for the increase of Catholics in this country; and I would not be so censorious as to say, that any description of people would bring their children up Catholics merely to avail themselves of a cheap and good education, but as I have been personally acquainted with many men of science, who have brought their children up Catholics of late, this fact, in itself very curious, in our times, naturally called my attention to the comparative advantages of an education received at one of our universities, and one received at one of the Catholic colleges of Stonyhurst or Ascott, and I find that the balance of advantage, both as to expense, literary attainments, and moral conduct, preponderates so greatly in favour of the Catholic colleges, that I am induced to hope, now that London is likely to have a large and liberal university on a better footing, unshackled by any old antiquated tests of faith, that its learned institutors will take into their particular consideration, how much some general school for young medical men, conducted on a liberal plan, and with as little expense as possible, is really wanting in these days of general improvement.

Yours, &c.

ACADEMICUS.

May 4, 1827.

THE LANCET.

London, Saturday, May 12, 1827.

THE attack of Sir Anthony Carlisle on the general practitioners of the British empire, supplies another illustration of the happy constitution of the English College of Surgeons. When the charter was granted by his late Majesty, we have no doubt that he was actuated by the best of motives; influenced by an anxious desire to diminish the sufferings of his subjects, by protecting an institution dedicated to the cause of medical science. His Majesty, however, could not have anticipated such results from his benevolence, as we now experience; he could not have expected, that those whom he intended to advance the cause of science, and to protect the rights, privileges, and immunities of their professional brethren, should have been the first to accomplish their destruction, and to destroy the only foundation on which the surgeon can rest—a substantial hope of success, viz. his professional and private character.

If the accusations promulgated by Sir Anthony Carlisle had been the effusions of some acrimonious and malignant creature, altogether unconnected with our profession, they would even then have excited disgust in every honourable mind; but considering that they emanated from an individual who is one of the Court of Examiners of our college; a man, who, on entering office, swears to "protect and defend *every member*, in the exercise and enjoyment of his rights, privileges, exemptions, and immunities;" disgust gives place to feelings of honest indignation. The enemies of reform, on a variety of occasions, have not been backward in exhibiting facts, which prove, most incontestibly, the accuracy of our arguments relative to the state of the college government; and those who have advocated the cause of the "ruling powers," from an affected disbelief of our statements, have been driven from their hypocritical incredulity, by the absurd and malignant acts of their most stupid coadjutors. If the Council of the College were elected *annually* by the MEMBERS, would any one of them have dared to insult the great body of the profession in the manner of Sir Anthony Carlisle? No. Yet this man, who has accused the Members, not only of violating the most sacred of trusts, and even of murder; aye, of murder! still holds his office in the College, and WILL hold it, unless the mode of election be altered, in despite of the detestation in which he is universally held. What opinion must the public entertain of GENERAL PRACTITIONERS, when Sir Anthony Carlisle, one of the heads of our College, tells "HIS MAJESTY'S JUDGES, CORONERS, AND JUSTICES OF THE PEACE," to watch the conduct of Surgeons, and to drag them before a tribunal of their country, when death is connected with the employment of surgical instruments? What opinion, we ask, must the public entertain of the talents and integrity of the profession, when addresses of this nature, from Sir Anthony Carlisle, are circulated throughout the kingdom in *The Times* newspaper? Sir Anthony Carlisle either does, or does not know, that the atrocities of which he speaks have been committed; if the first, what steps did he take to bring their authors to punishment?—if the second, what are we to think of a man who, in the absence of all evidence, attempts to blast with one stroke of his pen the reputation of his brethren—the reputation of those whose interest he is *sworn* to protect? The immediate cause of Sir Anthony's resentment may be traced to his being in a minority at the College when the following resolution was carried:—"That from and after the 1st January, 1828, every candidate for the diploma of this College will be required to produce certificates of having attended two courses of lectures on the obstetric art and science, delivered by

competent teachers, in addition to the certificates of professional education at present required."

This measure was adopted doubtless from the pointed allusion to the practice of midwifery contained in the SURGEONS' PETITION; it is, however, not a little curious that the Court of Examiners should require certificates of attendance on obstetric lectures, whilst they have a BY-LAW which excludes the OBSTETRIC PRACTITIONER from a seat in the council; but this, with *such* an executive, is only a trifle. A matter of somewhat more importance presents itself, when we reflect, that *in consequence* of this by-law the court does not contain a single examiner who is capable of instituting such an examination as would protect the public from the unskilful hands of an ignorant operator. This is only one of the thousand considerations which cry aloud for SURGICAL REFORM; but it is not our object on this occasion to discuss the college question, and we have only noticed it incidentally, to show that a man can retain the highest office in the college, and, *with the certainty of retaining his seat*, vituperate with unparalleled falsity and malignity, nearly the whole of its members. That his accusations are false, must be evident to all; because, from the *spirit* perceptible in his communications, it is but too obvious that if he were acquainted with any cases of malpractice, nothing would be more gratifying to his disposition than to give them publicity; yet, in the utter absence of proof, this man is foolish enough to believe that his statements will be credited. Sir Anthony's arguments, if they can be so called, against the cultivation of obstetric surgery, are no less absurd and unfounded than his attacks on the practisers of the art are slanderous and base. He tells us that labour is "a purely natural process, so wisely ordered, that it very rarely demands any other aid than experienced mothers can safely give." We admit, to the fullest extent, that the birth of a child is "a natural process," and that, in numerous cases, an experienced mother could render every requisite aid; but the *rarity*, good Sir Anthony, how is *it* to be treated? *because* the birth of a child is "a natural process," would you not instruct the student how he might overcome a difficulty? or when the difficulty does present itself, would you deprive the poor suffering mother of surgical assistance, simply *because* child-bearing is "a natural process?" Why, good Sir Anthony, is not *walking* a natural process? yet occasionally a man, in his perambulations, tumbles into a pit, or slips from off a curb-stone under the wheels of a wagon,—yet, *because* walking is a natural process, would you deny him the assistance of surgical aid to replace his fractured and dislocated bones? Is not *seeing* "a natural process?" yet, *because* "not one in a thousand" is affected with cataract, would you refuse the blind the aid of a surgeon, *because* seeing is a natural process, and "not one in a thousand" is afflicted with blindness? And although child-birth, with the generality of women, is accomplished without the assistance of art, yet it occasionally happens, much more frequently, indeed, than "once in a thousand times," that the greatest decision, skill, boldness, and dexterity, on the part of the practitioner, are required to secure the safety of parent and offspring, which are sometimes insufficient to preserve one from death, sometimes both, and now and then the preservation of the parent demands the sacrifice of the offspring. This is a scene of anxiety, evidently unknown to Sir Anthony Carlisle,—a scene so chequered and trying, so pregnant with difficulties, so intimately associated with the finest sentiments of our nature, requiring so much judgment to pass through with success, so harassing to the feelings of the surgeon, and of responsibility so tremendous, that the gratitude of the unhappy

sufferer, and her sympathising relatives, can prove the only satisfactory return to the practitioner for his anxieties and exertions.

When a difficulty really occurs in the course of labour; when a deviation from the "natural process" does present itself; Sir Anthony, with truly characteristic disinterestedness, contends, that the case should be submitted to the discretionary judgment "of Hospital Surgeons or Hospital Physicians of enlarged intellect." If the public suffer already through the ignorance of Medical Practitioners, it will not go much towards an improvement of their condition to be placed under Hospital Physicians or Surgeons. Does not Sir Anthony recollect the case of Catherine Owen, which occurred at St. George's Hospital, under the care of the "enlarged intellect" of the *Professor of Medicine at the University of Cambridge*? The treatment that poor woman experienced, we apprehend, is a fair specimen of the advantages which English females would derive from being subjected to the influence of the "enlarged intellect" of Hospital Physicians and Surgeons. Let any man read the pages of this Journal, and then say whether the Hospital Physicians and Surgeons of this town have "enlarged intellects," and are entitled to a greater portion of confidence in *any* department of the profession, much less in the obstetric, than General Practitioners. Do not the by-laws of the College exclude from the Council those who practise Midwifery or Pharmacy; and do not Hospital Surgeons who are not already of that Council, submit to the law in order that they may not disqualify themselves? yet these men of "enlarged intellect," who have always scouted the practice of Midwifery, are, according to the statements of Sir Anthony Carlisle, better qualified to contend against the difficulties of preternatural labour than general Practitioners, who make the science an object of constant study and investigation. Hospital Physicians and Surgeons, of "enlarged intellect," indeed! hospital jackasses would be a much fitter term, if it be intended as a guide to the measure of their "enlarged intellects." Do Hospital Physicians and Surgeons, previous to their election, afford any proof of their "enlarged intellect?" it is notorious that they do not; and what evidence do they furnish after they are elected? why, speaking professionally, that they have not a single well-founded claim to the offices they hold. We could insert a pretty summary of the evidences of their skill, and if this oysteream swaggerer provoke us with any more of his effusions, we will furnish the public with a history of his scientific testaceous exploits in the wards of the Westminster Hospital.

We have much more to say on this subject, but want of space compels us to stop here at present.

Observations on the Structure, Economy, and Diseases of the Foot of the Horse, and the Principles and Practice of Shoeing. By Edward Coleman, Professor of the Veterinary College, &c. &c. 4to. pp. 128. Johnson, St. Paul's Church Yard, 1798.

The interest which we have taken in the veterinary art, has been excited by a deep consideration of its importance, not to a few only, but to all mankind.

Who does not derive, directly or indirectly, profit or pleasure, or both, from the willing services of the noble horse? and who should not feel it his duty, setting interest aside, to contribute towards prolonging his servitude in ease and comfort? When therefore the fact is apparent, that in this country which boasts of humanity the lives of these useful animals are usually abridged to one half their natural period, in a regular course of disease and suffering, chiefly by the destructive consequences of shoeing, we need urge no other apology, no minor argument,

than the broad course of public usefulness, in support of the part we have taken in discussing and unfolding this interesting and important subject.

Actuated by these feelings, we have lately been induced to give a favourable review of the works and opinions of Mr. Bracy Clark, and to avoid the charge of partiality, it will be proper now to consider the labours of other writers on the veterinary art.

What are the doctrines maintained by the Veterinary College? is the first and most natural inquiry; but in proceeding to answer this question it is necessary to observe, that the public have been misled by the delusion of a name, in supposing that this institution consisted of a body of learned and experinced men, whose opinions deserved the respect which we usually pay to the decisions of united knowledge. More minute observation has convinced us, that such is by no means the case; that this establishment ought, in some respects, rather to be considered as a private school, than as a free and public College, inasmuch as it is almost exclusively taught and governed by a single individual, to wit, Professor Coleman; who, in the triple capacity of lecturer, teacher, and examiner, is virtually the single master of the institution.

As an inquiry, therefore, into the doctrines and practice of the College must eventually resolve itself into the personal opinions of the Professor, we shall save time and argument, (having formerly shown the circumstances and manner of his elevation to the chair,) by referring at once to the printed works in which he stands committed, and afterwards comparing them with the substance of his present lectures, from which they will not be found very materially to differ.

The preface to the book before us is chiefly occupied in setting forth the advantages which the country, and the army-service in particular, had derived from the appointment of Veterinary Surgeons, instead of the ancient farriers; the errors of whose practice, and the admitted evil consequences of common shoeing, are ascribed to their ignorance of those principles which the author had discovered, and was about to unfold.

He observes,—

"The public have already derived some advantages from the College, in return for the liberal grants that have been made by Parliament. The horses of His Majesty's Most Honourable Board of Ordnance have been shod very successfully for two years, on the plan employed at the College, and the greater part of the British Cavalry are now shod in the same manner."

Now we can assert it as a fact, that if this ever was the case, it is not so now, there being no regiment in the service that is shod upon the principles of the College, which are those of frog pressure, which to be carried into real effect would require a low-heeled shoe, as proposed by its inventor, La Fosse, who himself afterwards abandoned it, nor was it ever after attempted to be resumed in France.—Ergo, (to use his favourite phrase,) either Mr. Coleman made too general a statement, or his "principles" have since failed him.

He then goes on to state, that "the practice here recommended preserves the structure and uses (of the foot) unimpaired by shoeing;" but our comments on this very confident declaration, must be reserved until we have investigated and analysed the means by which these desirable effects have been pretended to be produced; and passing over as unnecessary the prefatory remarks, which are chiefly in illustration of the above assertions, we shall plunge at once into his account of the Anatomy of the Foot, as the theory which he advocates, and the practice he recommends, must rest on its correctness or falsity.

Thus it begins, in his usual manner, with an undeniable common-place proposition:

"Before we consider the best practicable mode of preserving the feet of horses by shoeing, it will be necessary to describe the external form, the structure, and economy of the hoof. Those that are in the habit of

examining the structure and economy of animals, as they advance in knowledge, are more and more convinced that every part is made with infinite wisdom and contrivance, that each part has its peculiar use, and that had it been made otherwise, the purposes for which it was intended could not have been answered."

We should have no difficulty in demonstrating that a *circular* foot, as it comes from the *hands of the maker*, is the best possible form for the horse; and "as the natural hoof is created *circular*, any deviation from this figure produces an equal alteration of the parts contained within," for the following excellent and remarkable reason, "the cavity of the hoof is always completely filled by the foot, so that the natural hoof is perfectly equal to contain the foot, but not one hair's breadth larger or smaller" What an eminent discovery! It is to be regretted that it was not followed up, as he might have informed us by the same rule, that the brain fills the cranium, and that every animal exactly fits his skin. Again, "no shoe can possibly be fitted with such mathematical exactness to the human foot, as the hoof is to that of the horse." Surely, in a grave work on the elementary principles of his art, he might have spared such a frivolous remark as this.

Farther on, (page 24,) he says,—

"The whole of the hoof is composed of horny fibres, without the smallest degree of sensation. The crust or wall surrounds the anterior and lateral parts of the foot; it grows obliquely from the coronet downwards, and increases in width as it descends. It is smooth and convex on the outside, but *laminated* and concave within, for the purpose of being united with corresponding *lamina* covering the *lowest* bone of the foot, called the coffin-bone. This union of the crust with the coffin-bone, sustains the weight of the animal; and as the *laminæ* are elastic, this furnishes as many elastic springs as there are *laminæ*, to prevent shake and concussion when the horse is in action. The horny sole is united with the lower part of the crust; it is concave on the outside, beginning at its junction with the crust, and increasing as it advances towards the centre, so that the edge of the sole united to the crust is least concave. (Not very intelligible!) The sole, on its internal surface, is convex. The bars or binders, as they are called, *are two in number*; they are placed between the frog and the sole,—and, at the heels, form a broad solid junction with the crust. The use of the external bars (are there any internal ones?) is to keep the heels expanded, and the internal laminæ of the bars are intended to prevent dislocation or separation of the sensible sole from the horny sole. In the natural foot there are *two large cavities* (of no definable shape or dimensions, we suppose) between the frog and bars."

Such is Mr. Coleman's description—is it worthy the head of the Veterinary College? But our readers must remember, that he never was a veterinarian, although he had been four years professor. But the want of a proper education, though it may palliate, yet cannot excuse, such egregious ignorance of the real structure of the horse's foot as manifests itself in every page of this curious volume; it is evident that he was not aware of the formation and real uses of the bars, being inflections only of the wall or crust as he calls it (a bakerly idea); and those parts which are so repeatedly mentioned under the term *laminæ*, instead of being horizontal plates or layers resting on each other, as the name implies, are, in reality, the perpendicular and oblique processes, which connect the coffin-bone with the foot.

Would not any school-boy have said, in general terms, that the horse's foot was round? why then does he arrogate its circular form to himself as a discovery, and ignorantly fail to notice those most material deviations from that figure in the natural foot, (vide Mr. Bracy Clark's plate and description,) which we would recommend to the particular attention of the veterinarian? By the "circular foot, as it comes from the hands of the maker," therefore, he cannot mean the colt's foot or the natural one, but the foot as it comes from the hands of the farrier, who actually does his worst, at the first shoeing, to reduce it to this figure; and, indeed, by referring to Mr. Coleman's plates, we may see that the blacksmiths' improved

cut has been the professor's guide; for there, within a circle as correct as compasses could describe it, we find delineated the perfect foot,—we may say, the perfectly false and unnatural foot of this sapient teacher.

But his description of the frog, his favourite organ, may well account for the mistaken views he has taken of its real functions, and the failure of all those successive schemes, in which this poor frog was to take so *prominent* a part, and per force, to act or suffer.

Page 28, he says,—

"The frog is an insensible body, placed in the *centre* of the sole, (by no means,) of a wedge-like form, pointed towards the toe, but expanded as it advances (retrograding from its central position, we suppose) to the heels. In the centre of the broad part there is a *fissure* or separation. (By this inapplicable phrase, he must mean the deeply indented oval base of the frog-stay.) The frog is connected internally with another frog of a similar figure, but different in structure. The internal frog is much more elastic than the horny frog; it has sensation, is connected above with a small moveable bone, by some called the shuttle-bone,—(gross blunder, they are not connected in any case,) and, at the extremity of the heels, with *two elastic substances called cartilages.*" (Here, again, we have a specimen of anatomy; the cartilages are at the sides of the foot, attached only to the coffin-bone, and half a mile, figuratively speaking, from the internal frog, to which they could not be united, without manifestly injurious consequences.) The toe of the sensible frog is united to the coffin-bone, and is a fixed point, but more than nine-tenths of both frogs are behind the coffin-bone, and the heels being in contact with moveable elastic (and not fixed or resisting) substances, a very considerable lever is formed, and whenever the hoof comes in contact with the ground, the frog first ascends and then descends."

How are we to apprehend a lever of soft, elastic, and yielding materials. "The ascent of the frog expands the cartilages, preserves the heels from contraction, and affords to the horse an elastic spring, while its wedge-like form prevents the animal from slipping, whenever it embraces the ground. But the shape and convexity of the frog clearly demonstrate, that it was formed to come in contact with the ground." Undoubtedly, in some degree; but we are now arrived at a point at which some explanation will be necessary, touching this darling college doctrine of pressure on the frog. Apparently unacquainted, when he published, with the proper bearings of the foot on the soil, (since so ably shown in the works of Mr. Bracy Clark,) and that this organ, in natural feet, was removed considerably from the wearing line of the wall, receiving pressure only in a third rate degree; it appears to have been the object of the professor's life to invent *patent* shoes for the purpose of bringing a very large share of the horse's weight on this soft and retiring organ, which the above author has well described as the passive bowstring to the elastic bow of the hoof, permitting, yet at the same time restraining, its action. The situation, structure, and attachments of the frog, will clearly show that it was not designed for the active function of expanding the hoof, which, when unconfined by the common shoe and nails, is itself, in the very principle of its construction, sufficiently elastic; and being placed, to use Mr. Coleman's own words, "in contact with moveable, elastic, and not fixed or resisting substances," it can possess no power equal to such an object. "Shall we then doubt," says the Professor, "that the frog is made with the same degree of wisdom as other organs? Shall we not conclude that it was intended to receive pressure, since its convexity must make it liable to touch the ground at every step?" Certainly; Mr. Clark admits, and no man can reasonably doubt, that the frog is perfectly capable of sustaining all that nature and its situation require of it, and occasionally much more, but that it will not bear that unremitted unnatural pressure, which the Professor would impose; why, it is proved by the following facts, without any appeal to reason, that all four of his frog-squeezing patent shoes have

totally failed, and that not one pupil of the hundreds he has taught can now be found *practising* these doctrines, as every regiment in the service can bear witness.

For, if to be twisted into every variety of fantastic forms—to be patented, patronised, and pushed by the Veterinary College for a period of twenty years and more, with all the advantages the British army could afford as a field for experiment; if this be not considered by every unprejudiced reader, a fair and proper trial of the false principle of the whole tribe of frog-squeezing and spit-bar horseshoes, we can no longer appeal to facts or experience in support or refutation of a system.

When facts are so conclusive, and against the shadow of a past theory which time has tried and condemned, it would be needless and unavailing to draw the weapons of argument; we reserve them, therefore, until they are provoked, and must again revert to our author:

"The frog," he says, page 33, "it can be demonstrated, was not made to defend the tendon, (the perforans, which is inserted beneath the coffin-bone,) because the substance of tendons in health has no sensation;" inferring thereby that it might have been as well unprotected, or that the frog could not at the same time perform two functions. Could this unrivalled physiologist, as one of his panegyrists dubs him, be so ignorant as not to know that an undefended tendon, liable to be bruised, would inflame, and quickly become the most sensitive part of the body; or in this, as in other instances, he might have forgotten, with his characteristic partiality for an abstract proposition, that the rule did not apply; that though tendons had no feeling, they might possibly acquire some by battering on the road. But we need follow his description no further, having reached the point to which all his argument tends, and found the important doctrine which every succeeding page enforces, so let those who would know all, hear in one word, it is pressure, pressure on the frog.

A merciful desire to render every justice in our power, has its share in inclining us to abstain from exposing any more of the manifold errors which *crowd* these pages; but from the second part of this work we propose to make more copious extracts, illustrative of the writer's opinions, style, and mode of reasoning. It was published four years subsequently to the first, when he had been eight years professor; and we may presume, therefore, is a more matured, digested, and better exposition of his sentiments.

We shall conclude by describing the kind of shoe which, in strict conformity with the above principle of frog pressure, is most strenuously recommended throughout the present volume. It is the same as that which was previously adopted by La Fosse, a French veterinary writer, and though quickly abandoned and forgotten on the Continent, it was long known amongst us as the "*thin-heeled shoe*," being three times as thick at the toe as at the heels, in order, to use Mr. Coleman's *elegant* and *forcible* language, that it might "bring the frog completely into contact with pressure." This effect it fully accomplished; in other respects its principle was precisely the same as that of the common shoe; being fastened by nails at the quarters, it equally prevented and destroyed the expansive action of the foot, and consequently induced contraction, with all its attendant evils, in an equal and similar degree. With all the objections which apply to the common shoe, it had others peculiar to itself, and which have caused it to be wholly laid aside in the present day. Its unequal thickness occasioned the horse's weight to be thrown too much backwards on the heels, which sometimes became bruised and tender, but more often its use was followed by strains of the flexor tendons, or back sinews, and this

practical disadvantage soon overbalanced the Professor's authority in public opinion. So ends part the first, of this farce of shoeing; and after 30 years of uninterrupted failure, with every advantage in his favour, is it not surprising that we can patiently see the performer bringing forward new plans on the old principle—frog pressure?

LONDON MEDICAL SOCIETY.

Dr. Haslam, *President, in the Chair.*

At the meeting of Monday last Mr. Iliff introduced the subject of hydrophobia, principally with a view of showing that the recommendation of using the cupping glass to prevent absorption of the rabid poison had been recommended by Mr. Hume, chemist, of Long Acre, in 1804, and to obtain some explanation of the motives which induced the medical officers of Guy's Hospital to adopt the desperate step of amputating a man's arm after the constitutional symptoms of the disease had manifested themselves.

The case alluded to was that published in The Lancet of last week, and Dr. Haslam, seeing Dr. Wa'shman and Mr. Callaway present, requested that they would be good enough to relate what they knew of the case. They did so; but as their account so resembled that previously given in the Lancet, it is unnecessary to repeat it. Mr. Callaway said, in explanation of the course adopted at Guy's in the above-named case, that the amputation of the arm was suggested by Drs. Back and Bright, the physicians of the hospital, and acceded to by the other medical officers, as a *mere experiment*, founded upon the theory so strongly contended for by Dr. Barry, of the lodgment of the poison in the part bitten, and of its gradual absorption on the appearance of the general symptoms. The position of the wounds were such as to prevent the cupping glass from being applied, and the result of the consultation was that the amputation should be proposed first to the friends of the patient, as an experiment, stating that all former modes of treatment had proved useless. Their consent having been obtained, the nature of the case was submitted to the patient, who, like any other dying man, was ready to catch at a straw, and so the limb was removed. Mr. Callaway observed, that whatever opinion might have been before entertained respecting the theory on which the performance of this experiment rested, the result of it must serve to convince every body that the notion of the rabid matter being retained in the part up to the time of the breaking out of the hydrophobic symptoms, was altogether fallacious, and *undeserving of further attention.*

Mr. Drysdale mentioned that the proprietor of a nostrum called the "Berlin Drink for the cure of Hydrophobia," had reported it to have cured three persons in the neighbourhood of Kennington; but Mr. Drysdale observed, that the advertiser had forgot to mention that these persons, bitten by a dog supposed to be mad, had applied to him previously to their taking the "Berlin Drink," of which they drank seven guineas worth, and that he had excised and cauterized the bitten parts. No symptoms of hydrophobia had appeared in these persons so reported as cured.

The President observed, that such persons usually forget to mention every thing but that which served to convey money from the credulous into their own pockets.

Mr. Callaway thought it a fit opportunity of suggesting to the society the formation of a committee, according to the custom of the French, for the purpose of taking into consideration the mode of treatment most likely to be useful; the opinion of which committee might be a sort of direction and sanction to the members of the society generally to act upon if they should be called to a case of developed hydrophobia. Respecting the best prophylactic measures there was now no difference of opinion; but he confessed candidly, that if called that night to a case of this kind he should hesitate as much as ever what course to pursue, and he believed that much valuable time was often lost by such hesitation.

Mr. Gossett, Iliff, and others, seemed to think that much harm was done by attempting too much in the general anxiety to do something; and here the business of the evening closed.

ROYAL ACADEMY OF MEDICINE.

The second public annual sitting of this Academy took place yesterday, in the palace of the Louvre. Baron Portal, principal physician to the king, and Baron Dubois, presided.

Five reports were read: 1st, on the epidemics of 1825 and 1826, by M. Pariset, secretary to the Academy; 2dly, on the labours of the committee of inquiry, on the subject of mineral waters; 3dly, on the subject of secret remedies, by M. Husson; 4thly, on vaccination, in the name of the committee appointed to inquire into this important subject, by M. Dubois; and,

5thly, on the prizes, by M. Adelon. The epidemics of 1825 and 1826 are ascribed in the Report of the committee to unhealthy habitations, and to masses of filth collected in their vicinity; to water impregnated with particles of poisonous substances, and to the means commonly employed by people in the provinces by way of remedies,—such, for instance, as the administration of wine in large quantities to patients whose condition requires an antiphlogistic and tranquilizing treatment. M. Pariset has found that the morbid affections produced by these epidemics, have commonly been seated in the mucous membranes.

The remarks of M. Ferrus, on the subject of mineral waters, might, for ought we know, have been novel and sagacious, but we cannot speak with certainty upon this point, for they were as inaudible as the Cruikshankian oration.

M. Dubois replied triumphantly to the objections raised by ignorance, prejudice, and bad faith, against vaccination. He demonstrated, both by reasoning and by facts, the immense benefit which France had derived from this preservative, and bore testimony to the merits, not only of Jenner, but of the late Duke de la Rochefoucault Liancourt, who, according to M. Dubois, introduced vaccination into France.

The prize-subject this year was the doctrine of absorption. Two papers had been presented, but neither of them having fulfilled the conditions required by the Academy, no prize was awarded. The Academy has simplified the question, by proposing the following subject for a prize of a thousand francs: to show, by experiments and cases, the effects of the mechanical means calculated to prevent the absorption of deleterious substances in general, and especially that of the hydrophobic virus.

After the Reports had been read, M. Pariset pronounced an eulogium on the late M. Pinel, a physician whose able contributions to medical science are known to the profession throughout Europe.

Paris, May 3, 1827.

ST. MARY ABCHURCH,

Lombard Street.

A VESTRY was held in the above parish, on the 2d inst., ostensibly for the purpose of passing resolutions, declaring the opinion of the meeting, that the reports of two former meetings of the vestry, inserted in THE LANCET, were incorrect, and which our readers will recollect tended to cast a strong reflection of inhumanity on the overseers towards the poor. The subject has excited a lively interest in the parish, and the vestry room was crowded to excess.

On taking the Chair, the Rev. Mr. Bellamy produced THE LANCET, and was about to make some observations on the reports above alluded to, when

Mr. Rees Price said, he had heard that the parish had been canvassed, in order to the adoption of resolutions to the effect that the charge against the parish officers was untrue.

The Chairman asked whether he had canvassed the parish. Only a few voices answered in the negative.

The Chairman said, he had read with surprise the reports of the two last vestries in that *chaste** publication, THE LANCET; and, in a speech of considerable length, endeavoured to show the inaccuracy of the reports in question.

Mr. Price. The reports are substantially correct. If any ought to complain, it was himself; as he had been made to state that the poor woman Paisley had died in a few days, whereas he had said she died in a few hours after removal to the workhouse. With respect to the appellation of "that vile publication," given by the Chairman to THE LANCET, he would only state that the work was so extensively patronised, that its sale amounted to nearly 12,000 per week.

The Chairman. I said "that *chaste*," not "that *vile*," publication.

Mr. Gorton. As far as respects myself, the reports are correct.

A scene of much noise and confusion ensued, in the midst of which the Chairman accused Mr. Price of being the author of the reports, which the latter denied.

The Reports in THE LANCET were then read, when it appeared that the only inaccuracy in them, was the placing one speech before another, not very surprising, when it is known that two or three persons were addressing the Chair at one time.

Mr. Meggison, the druggist, was in the habit of travelling round the country, and every where met with THE LANCET. It was a most useful publication; and from its extended circulation, was well calculated to give publicity to every subject. The care of the poor occupied the attention of every parish in the kingdom, and any proceedings relative to a better provision for the sick poor, would be read with interest. With respect to the amount claimed by Mr. Price, for attendance on paupers, he would suggest that legal advice should be taken, rather than 100*l.* should be expended in litigation.

Mr. Dawson, with much *gravity*, then rose to move a resolution that the Editor of

* The *Reverend* Chairman might well use the word *chaste* ironically, as THE LANCET, on one occasion, contained the virtuous exploits of a *Parson*.—ED. L.

The Lancet be *ordered* to attend the next Vestry, and state who were the authors of the Reports! Mr. Woodward readily seconded the motion.

Mr. Meggison. It is ridiculous to suppose that the *order* will be attended to. Mr. Editor is too much occupied with more important business. The question was not put from the Chair.

Mr. Woodward defended himself from the charge of inhumanity, and repeated that he had acted under the advice of Mr. B. Atkinson, in removing the woman Paisley.

Mr. Price. Symptoms of mortification were evident, of which every practitioner ought to be aware. The poor family who laboured under the small pox was kept without assistance from the parish, from 28th of September to the 3d of October. Was that humanity?

The Chairman. If Mr. Atkinson was wrong in his judgment, Mr. Woodward ought not to suffer odium for it.

A Resolution was then passed, exculpating Mr. Woodward from the charge of cruelty to the paupers.

Mr. Price. Now we see the result of the canvass. He did not make a charge of cruelty, but of gross neglect.

A protest, before handed in by Mr. Price, relative to the expenditure of the parish money, caused some discussion during the meeting, which was a very noisy one. A special vestry is to be called, to consider the protest. Mr. Price's claim remains to be settled by a court of law.

To the Editor of The Lancet.

Sir,—By giving insertion to the following, you will oblige

An Independent Physician.

From a Physician in Town to a Student on the Eve of Graduation.

My dear S——, Understanding that it is your intention, after having finished your education, to settle as a physician in London, and that the period is not probably very distant, I think it may be useful to you to know something of the present state of professional matters here, as far as they are comprehensible, or I am capable of explaining them. In the first place, it is not easily to be comprehended that, whilst the actual number of physicians in the metropolis does not exceed one-sixth part of their due proportion to the population, their branch of the profession is still very far from being lucrative. Such, however, is the case; and without entering at present into an explanation of these surprising facts, (for which, in the mean time, consult recent publications,) let me observe, that I do not by any means state them as a discouragement from the execution of your purpose. On the contrary, I am persuaded that the nearer the number of physicians approaches to its due proportion to the population, the more generally and equally successful, and the more independent and powerful will they become, provided they adopt the means which are here recommended to them, and are actually within their reach, for regaining their alienated privileges and nullifying existing monopolies.

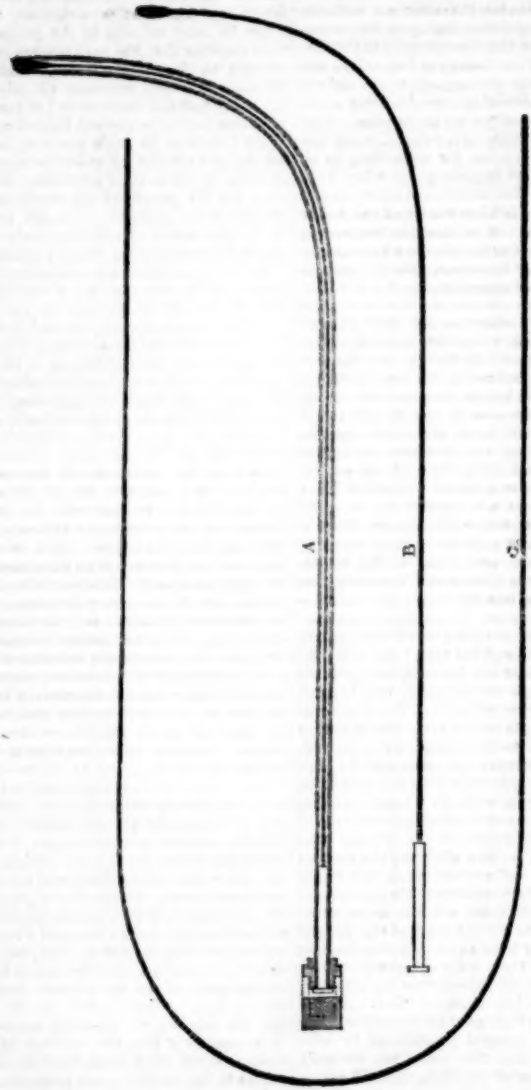
Having completed your studies, undergone your examinations, and obtained your testimonials of fitness, you no doubt think yourself authorised, in virtue of your diploma, to exercise your profession 'ubique gentium,' throughout the world; and so you undoubtedly are, and it depends wholly upon yourself to assert and to maintain your own rights and privileges as a medical graduate, as well as the rights and privileges of your University. But in order that you may be enabled to do so effectually, it is necessary that you should be informed beforehand of of the manner, and the quarter, and the grounds, upon which they are considered as liable to be assailed, upon your fixing yourself as a practitioner in this great city. I say "*considered* as liable to be assailed," because, in point of fact, there exists no power by which you can legally be prevented from exercising your profession in London, any more than in any other part of the British dominions, upon the only authority recognised by the law of the land, by reason, and by common sense,—that of your diploma. Do not, therefore, allow the erroneous belief to influence your resolves, that the body of learned monopolists, called the Royal College of Physicians in London, possess any power whatever of prohibiting graduates in medicine, *as such*, from freely exercising their profession. If these monopolists were not conscious that they do not possess the shadow of any such power, would they not endeavour to exercise it against the scores of independent physicians who are now settled in and around the metropolis? Why then should the graduates of the northern Universities of the kingdom, unnecessarily submit to the indignity of a second examination, to the sacrifice of paying 65*l.* 13*s.* 4*d.* for a licence to practise medicine? and what is worse, to the degradation of being placed in a subordinate professional rank by the graduates of the Universities of the south? Why should they condescend, in thus humiliating themselves, to perform the immoral and disgraceful ceremony of swearing, *on their bended knees*, to do all things in honour of those who degrade them, and even to observe by-laws, *with which they are not permitted to be made acquainted*? All this, if it were not of frequent occurrence, would be deemed incredible. It is, indeed, quite

impossible to believe that such indignities should have been so long submitted to, and by persons too in the character and with the education of gentlemen, but upon the erroneous supposition that a conformity to the illegal by-laws of the College of Physicians was an indispensible preliminary to the right of settling with advantage, or of settling at all, in the practice of physic in London. Now even this necessity, if it existed, would not afford a valid excuse for submitting to all the immoral and degrading acts which have been stated. But it does not exist; and it only requires a full knowledge of this fact to make it a piece of inexcusable baseness in any man hereafter to become a licentiate of the College of Physicians. Such a step is not only not indispensible, but it is even not useful. Let us consider the further immoral acts, of which, after having undergone all this degradation, a licentiate must be guilty, under the reign of the College and the present regime, before he can hope to thrive, or even to live by his practice. Generally speaking, it is quite in vain for him to expect that he will, in any reasonable time, by an assiduous and conscientious application of his acquired knowledge, obtain such a proportion of business as will afford him a genteel or even a competent income. To ensure success under this system, there is only one line of proceeding to be adopted; and a very base one it is. Owing to the monopoly of the College of Physicians, that class of medical men known in other countries as apothecaries or pharmaciens, have succeeded in engrossing almost all the ordinary medical practice of the town; and it is only when the patient has been duly drugged into danger by these functionaries, that the real physician is ever called in. The choice of the physician, in such a case, almost always depends upon the apothecary; consequently, the apothecaries are the patrons of the physicians. The physician who happens to be on friendly terms with the greatest number of those keepers of the health and secrets of families, will of course be the oftenest consulted. But, besides obviating the responsibility of an unfortunate issue, and giving occasional entertainments to his patrons, the unfortunate physician will also be expected to prescribe for the interests of the general practitioner at least as much as for those of his patient. Upon a due observance of this policy it may depend whether his employer consults with him a second time. As the fellows of the College of Physicians are supposed by the general practitioner to have some power, and this *notion* has certainly been the cause of elevating himself into a patron, he defers to them more than to the licentiates, and *ceteris paribus* will in preference call them in. Hence the licentiate, in order to secure a portion of the favour of their common patron, must be doubly obsequious. Instead of the simplest, most efficient, and cheapest prescriptions, which would be most suitable to the patient, he must consider that the very reverse is most suitable to the apothecary. Accordingly, the more fully you renounce all principle, the better will you succeed in London as a physician under the present blessed regime. What physician in high practice, let me ask, has not felt himself under the necessity, in order to retain it, of prescribing fully as much for the benefit of the apothecary as for that of the patient? I am not treating of the immorality of individuals—it is the turpitude of the system which I condemn.

But are we under any necessity of conforming to this vile system? Certainly not. Can we not ensure success by practising honestly, conscientiously, and independently, not only without the patronage of the apothecary, but by limiting him, as in all other countries, whether civilized or barbarous, to his proper functions of dispensing medicines? This also we can undoubtedly effect, by means of a general union of independent physicians, not only keeping scrupulously aloof from the vortex of the system, but bidding open defiance to its unfounded pretensions and usurpations. For such a change the public are in the course of being fully prepared. They will soon learn the benefit of transferring their allegiance from the monopolist of the College of Physicians, and his ally the general practitioner, to the independent physician, and the dispensing apothecary, under the name of chemist and druggist. To promote this desirable change, a certain number of independent physicians in London have formed themselves into an association, to which medical graduates of all universities are eligible, wherever situated. Some of the leading objects of this association are to afford to its members a rallying point, and to support and aid them in all their lawful pursuits, as well as to protect them against all unjustifiable or unfounded pretensions whatsoever. Thus the young physician, settling in London, or in any other part of England, will enjoy efficient protection; the practice of physic and its cultivators will be emancipated from the double thraldom of the College of Physicians and the *practising* apothecary, and justice rendered to the public. The benefits of becoming members of an independent body like the FACULTY OF PHYSIC,* on the one hand, and the disgrace of becoming appendages to a monopoly like the College of Physicians, on the other hand, must be now apparent to the world. I am yours, &c.

London, May 4th, 1827.

* Vide "*Circular*," LANCET, Vol. XI, p. 579-83.



A. A section of the Catheter, with the stilet introduced.
B. The Stilet.
C. The Pewter Director.

DESCRIPTION OF A NEW CATHETER AND GUIDE.

By Henry Penneck, M.D. Penzance.

To the Editor of The Lancet.

Sir,—If you deem the two following papers, with a sketch of the instruments, deserving a place in The Lancet, I shall be obliged by your inserting them.

The first paper, with the drawings, I sent my friend, Dr. Clutterbuck, in September last; he read it before the Medical Society, when several of the members were pleased to express their approbation.

In several diseases of the male urinary passage it is allowed to be necessary for a time, to keep a catheter in the bladder, which often requires to be withdrawn for the purpose of cleaning it, and in attempting to replace it when much disease has altered the passage, rendered the parts irritable, and thrown them into spasmodic contraction, considerable suffering has been occasioned to the patient, and difficulty experienced by the surgeon. It has appeared to me, that the alterations and additions to the catheter which I am about to propose, will relieve the patient from much distress, and greatly facilitate the operation of replacing the catheter in the bladder.

The catheter is made of elastic gum. The stilet is of steel wire, and when bent to a proper curve, will be stiff enough to enable the catheter to preserve that figure, and sufficient to overcome resistance in passing the urethra, yet in a certain degree flexible from the curved part, that it may be more easily withdrawn when the catheter is in the bladder. The stilet has a a ball at the end to fit the opening in the extremity of the catheter, a brass cylinder is screwed on the stilet, for the purpose of regulating its length; this cylinder has an enlarged end to prevent the stilet projecting too far; thus it will be like the perforator of a trocar; a cap is screwed to the catheter over this cylinder, which forcing on the stilet and resisted by the enlarged end of the cylinder, will keep it firmly in its place, and combine the parts into one instrument. The extremity of the catheter will then present the same figure as the common one, and being passed into the bladder, the cap is unscrewed and the stilet withdrawn; as often as the urine is expelled the cap is replaced. When it is necessary to clean the catheter, I desire the patient to retain his urine for a time; I then unscrew the cap, and if I think it necessray to remove any mucus the catheter may contain, I apply the tube of a common two-ounce syringe, fitted to the catheter, which by a single exhausting stroke of the piston will bring such a gush of urine through it, the patient assisting with the expulsive effort, as certainly to clear the catheter. I afterwards use a kind of bougie, which I call a director; it is made of soft metal, (pewter,) or of elastic gum, and about one inch more than twice the length of the catheter, and so large as only to pass easily through it, and it is marked at the length of the catheter. I introduce this director through the catheter till the mark is close to it, which assures me it must have reached the bladder. The catheter should then be slid back and withdrawn, whilst the director is kept steadily in the bladder, by an assistant holding the penis in one hand, and the end of the director in the other, and gently pressing the glans penis on the director. The catheter being cleaned, is to be repassed on the director through the uretha, until it is ascertained by the appearance of the mark that it has reached the bladder. When the director is withdrawn the cap is screwed on, and the operation is completed.

This method will prevent the catheter being diverted from the course of the urethra into any fistula or other opening, or from cutting against an enlarged prostate, or occasioning irritation in the urethra, or pain to the patient. It will also be found important on other accounts. The contents of the bladder will be discharged by a larger opening, which may allow clots of blood, or other matters lodged in the bladder to pass out, or they will be more easily withdrawn by the syringe, and fluids may be more conveniently injected and again withdrawn, after the manner of the stomach pump, and, on some occasions it may probably be the happy means of preventing the formation of calculi in the bladder,

Every surgeon must have remarked the greater difficulty in passing a very small bougie through a stricture, than one of a moderate size, perhaps from the greater flexibility and softening of the small bougie in its passage through the urethra, and from its often taking a serpentine form, insomuch that sometimes it has not been quite certain whether the bougie has or has not reached the bladder. Mr. C. Bell, in his Operative Surgery, vol. 1, plate 2, has shown how the point becomes reflected; it is also more liable to enter the lacunæ, and this must be of still greater consequence when a caustic bougie is introduced. It has occurred to me that an instrument similar to the flexible catheter, might be found very useful in passing small bougies through narrow strictures. Mr. J. Hunter, in his Treatise on the Venereal Disease, 4to, 2d, edition, page 128, finding

B. The Stilet.
C. The Pewter Director.

he had gained no advantage after six months trial with the bougie, says, "I conceived that I might be able to destroy the stricture by escharotics, and my first attempt was with red precipitate; I applied to the end of a bougie some salve, and dipped it in red precipitate. This bougie I passed down to the stricture, but found it brought on considerable inflammation all along the inside of the passage, which I attributed to the precipitate being rubbed off in passing the bougie; I then introduced a silver canula down to the stricture, and through this canula I passed the bougie with precipitate, as before." This attempt having failed, Mr. Hunter seems to have abandoned the passing a bougie through a canula, for that of a caustic confined in a port-crayon. The inconveniences attending it are well known, and have induced Sir Everard Home and Mr. Whately to use bougies armed with caustic; but as far as I am aware they have omitted the use of the canula, which appears likely to produce much good effect, and the alteration I propose in Mr. Hunter's canula (plate 3, figure 1,) is to have it made of soft pewter, or elastic gum, instead of silver, which seems too stiff, and if twisted often becomes rough. The plug of the stilet should exactly fit the canula, and have a shoulder to prevent it from projecting too far, and perhaps a brass cylinder, as the stilet of the catheter, to regulate its length. The canula may be two inches shorter than a common bougie. It may be passed down to the stricture straight or curved, with the stilett in it, according to the situation of the stricture. When the stilet is withdrawn, ever so small a bougie introduced through the canula, will certainly preserve its right direction, and bear precisely on the stricture. Thus greater facility and safety will be given to the use of bougies. I conceive this method is entirely distinct from the purpose for which Mr. Hunter intended the canula.

In the foregoing description of my proposed improvements of the male catheter, among other uses for which I designed it I mentioned the more easily injecting liquids into the bladder, and withdrawing them after the manner of the stomach pump, as likely to be useful in cases of extravasation of blood, &c. in the bladder. Since I made that communication, I have remarked that "M. Amussat, of Paris, in a case of retention of urine from stricture in the urethra, had passed a flexible tube as far as the obstruction; and then by means of an elastic gum bottle, gently forced a small quantity of fluid through the stricture, which relieved the obstruction and the urine flowed."

This idea once started, seems capable of being carried considerably farther; perhaps in every case of retention of urine, if from spasm, then immediately; and if inflammation should have been present after the usual means have been used, it seems right to attempt cautiously to inject warm water, not with an elastic gum bottle, but with a syringe, which may afterwards assist the expulsive effort of the patient in withdrawing it. Though dissections have shown that part of the urethra which lies between the neck of the bladder and the stricture to have been much enlarged by the increased force necessary to expel the urine, whilst the stricture prevented its free passage, yet the canula being kept in close contact with the stricture, and the syringe applied to it, will prevent any but the strictured part being distended in injecting the fluid, or by the exhaustion of the syringe during the expulsive effort of the patient, because then the pressure of the atmosphere will be taken off from the passage *only*, and thus the fluid will be forced through the stricture with so much greater power, and perhaps the over-distended part of the urethra will be allowed to recover its tone. I think it probable, that by frequently but cautiously forcing a fluid through the stricture, it will become dilated much, as if a sponge tent had been introduced; and that by this method, strictures may be almost entirely cured without the aid of bougies.

If other gentlemen, who have more opportunities than myself, should so far approve of this method as to put it in practice, I hope, with your permission, through the medium of your most beneficial and extensively circulated work, they will favour the public with the result of their experience.

I am, Sir, very respectfully,

Your humble servant,

HENRY PENNECK, M. D.

Penzance, May, 1827.

P. S.—As Dr. Clutterbuck had made my paper of September 20, 1825, public, I was surprised to observe that one of the patentees of the stomach pump, in a vindication of his invention, dated April 1st ult., should have confined the method of injecting liquids into, and withdrawing them from, the bladder to that instrument, whereas a common syringe of trifling expense is, as I had stated, fully equal to that purpose.

LONDON SURGERY.

To the Editor of THE LANCET.

SIR,—Of all the advantages which this great city affords to every class of professional men, those which it confers on the surgical student are of the most varied and useful nature. Tempted by the high reputation which London surgery has obtained, I have left that school which hitherto has nurtured me, to reap those rewards which are here bestowed with so liberal a hand; and being, as I hope, far advanced in my profession, I intend to take rather a general survey of the whole practice, than to follow the opinions of any individual. First I directed my steps to Guy's Hospital, so celebrated for the production of the best surgeon of the present age; and I take the liberty of submitting to you the remarks which occurred to me on the operations which were performed on the day of my visit. (May 1st.)

Mr. Bransby Cooper first cut out a diseased mamma, and astonished me not a little, after all the boasted perfection, by the manner which he followed. His cutaneous incisions were so very small, (in comparison with those I have been accustomed to see,) that he was obliged to dissect back the integuments before he could remove the diseased mass, although it was by no means large, and thus left a large oval pouch, which must tend to produce a high degree of inflammatory action, and favour the formation of purulent matter, independent of the great chance it affords of a recurrence of the disease in the cutaneous texture. The operator was much embarrassed in the removal, by his not making his preliminary incisions deep enough, and being thus obliged to cut, in the latter stage, from within outwards in a very awkward way.

In the schools of the "land of the mountain and of the flood," it is a maxim, as generally inculcated as practised, to make the first incisions free, and down to the strong fascia which invests the pectoral muscle.

Amputation below the knee was next performed, for a fungous ulcer of the heel, of large size and long standing; but I could not obtain any history of the case, as the surgeon made no exposition in the operating theatre, and the students who were around me seemed more intent about the method of removal than the nature of the disease. The circular operation was the one selected, and much to my surprise, for I had supposed that this method was scarcely ever practised in this amputation; the flap being for many and obvious reasons so much preferable. A very ingenious way of making the flap has lately been practised in Edinburgh, by Mr. Syme, by first making an angular incision over the tibia, and then piercing with the catline inwards and outwards; thus making the flaps from the sides of the limb. This, although certainly a difficult operation, affords a very beautiful and regular stump, and is unattended by any of those disagreeable consequences arising from too great a proportion of muscle.

The method of dressing stumps, in Guy's Hospital, differs much from that followed in Edinburgh, but there can be no doubt that the one adopted here is much preferable, although there are some parts of it which might, I think, be altered with great advantage. In Scotland, the surgeons very generally still smother the face of the stump with charpie and bandages; while in the dressing alluded to, the edges of the wound were retained in contact, by means of two broad pieces of adhesive plaster, without any other covering. Now, Sir, it appears to me, that if three or four narrow strips, applied at some distance from one another, were employed instead of the two large ones which completely cover the stump, there would be no chance of the collection and consequent burrowing of purulent matter, which must be the consequence whenever suppuration takes place, every exit being completely blocked up. One of our surgeons, and perhaps the most distinguished, (Mr. Liston,) is so much impressed with this opinion that he invariably employs stitches. His success is well known. During Mr. Cooper's operation a circumstance occurred, which many may consider to be of a very trivial nature, yet others would think it very reprehensible,—the trusting to a youthful assistant the management of the circulation in the limb. The consequence was, that when the large vessels were divided they poured out a considerable quantity of blood, which in some debilitated patients might have produced very fatal results.

Of the general arrangements in reference to the patient, assistants and nurses, and of the neatness and composure of the surgeons, I cannot express myself too warmly; and I hope that after the many scenes of confusion I have witnessed in our infirmaries, they will make a not less lasting than agreeable impression on my mind.

Should these few observations be deemed worthy of a place in your pages, I shall at another time offer to your notice any critical remarks that may occur to me.

With grateful feelings for the knowledge I have acquired by reading your Numbers,

I am, Sir,

Your much obliged Servant,

THE SCOTCH CRITIC.

May 4th.

WORKS RECEIVED FOR REVIEW.

Medical Botany, or Illustrations and Descriptions of Medical Plants. By JOHN STEPHENSON, M. D., and JAMES MORSS CHURCHILL, Esq. F.L.S. London, Churchill, 1827. 8vo.

This Part contains four Engravings, which are equally well executed with those of former Numbers.

Practical Observations on the General and Local Antiseptic Properties of the Chlorurets of the Oxides of Sodium and Calcium in the Practice of Physic and Surgery, and for Correcting the Putrid Effluvia of Decomposed Animal Matter. By Dr. HASSELL, Resident Physician at Boulogne-sur-Mer, late of the British Army. London, Longman and Co., 1827. 8vo. pp. 31.

Remarks on the Moral Treatment of Insanity. By THOMAS KNIGHT, of Hope House, King Street, Hammersmith. Smart, 1827. 12mo. pp. 16.

A Critical Analysis of the Memoir read by Dr. Barry before the Academy of Sciences on the 8th June, 1825, at the Institute of France, on Atmospheric Pressure being the Principal Cause of the Progression of the Blood in the Veins. By HENRY SEARLE, Surgeon. London, Callow and Wilson, 1827. 8vo. pp. 88.

Observations on the Importance of ascertaining the Rates or Laws of Mortality among Europeans by Chronic Diseases and Hot Climates, on the data necessary for constructing Tables to show the probable Duration of a Disease; with an Appendix. By GEO. FARREN, Resident Director of the Asylum Life Assurance Company. London, Hessey, 1826. pp. 102. 8vo.

Thought not a Function of the Brain; a Reply to the Arguments for Materialism advanced by Mr. W. Lawrence in his Lectures on Physiology. London, Rivingtons, 1827. 8vo. pp. 80.

LITERARY INTELLIGENCE.

In the Press, and speedily will be published,

Pathological and Practical Observations on Spinal Diseases, illustrated with engravings; also, an Inquiry into the Origin and Cure of Distorted Limbs. By EDWARD HARRISON, M. D. F.R.A.S. Edinb., formerly President of the Royal Med. and Royal Phys. Societies of Edinburgh, &c.

Mr. COULSON is preparing for the Press, a Translation of Edward's Manual of Surgical Anatomy, and Tavernier's Manual of Surgical Pathology, with Notes. To be published by Simpkin and Marshall.

An Introduction to the Science of the Pulse, as applied to the Practice of Medicine. By JULIUS RUCCO, M. D.

TO CORRESPONDENTS.

Communications have been received from Dr. SULLY—Mr. WALLER—Dr. STEELE—Mr. COCKS of Hatfield—and Mr. WANSBROUGH; also anonymous letters with the following signatures—P.—D. V.—CASTIGATOR—EDINENSIS—A STUDENT OF EDINBURGH—TYRO—A BOROUGH STUDENT—X.—G. W. N.—A PUPIL OF THE LONDON HOSPITAL—JUNIUS—T. B——tt.—PHILO—R.

PHILO is informed, that the Edinburgh diplomatists are entitled to practise *surgery* in England, and can recover claims for such practice in English courts of law.

Will a PUPIL OF THE LONDON HOSPITAL favour us with his name confidentially?

We should like to see one of the articles mentioned by G. W. N.

A BOROUGH STUDENT desires us to hint to Mr. Lizars, "that he has entirely omitted to give a plate, or any anatomical description, of the female mammæ, with the physiological and pathological observations."

We have inquired into the circumstance mentioned by our Correspondent, and very much regret that he should have been subjected to any inconvenience; it arose from a misunderstanding on the part of our publisher. Several editions of many of the volumes are out of print, but any of those in our possession can be obtained *separately*; amongst others the third edition, in one volume, of Vols. III and IV.

Under the peculiar circumstances described by TYRO, we advise him to confine his attention, during the first course, to dissections and lectures on anatomy. We are entirely unacquainted with the lectures of the Doctor to whom he refers.